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FREUD AND PSYCHOLOGY

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The death of Sigmund Freud[1] has reminded the world anew of his extraordinary role in the broadening and deepening of psychological inquiry. Yet his influence on psychology was a very small part of the total effect he produced on the intellectual life of his age. In some of the arts, in literature especially, the impress of his teachings is one of the distinctive marks of the day. In the social sciences, too, notably in anthropology and sociology, the signs of his influence are unmistakable and familiar. In psychiatry, as everyone knows, he effected a revolution. Problems of ethics and æsthetics have been reviewed in the light of his theories, and the implications of his doctrines have by no means escaped the attention of students of philosophy and religion. Even such impersonal disciplines as logic, mathematics, and the physical sciences take on, as human enterprises, a special significance when seen from his point of view. But most remarkable of all is his effect on the thought of everyday life. Much of the Freudian terminology has found its way into common speech; and a general, though vague, notion of his interpretation of human nature is part and parcel of the common thought of the age.

It would be a mistake to regard Freud's influence on psychology as constituting a problem essentially different from that of his influence on any other field. For it was not as a psychologist that Freud influenced psychology. Neither was it as a man of letters that he influenced literature, nor as an anthropologist that he influenced anthropology. In every

[1] Freud died in London on September 23, 1939.

case his action was that of a force from without. Even psychiatry is no exception to the rule, for it was not by working in the professional tradition that he exerted his influence on his own profession. His development in psychiatry was always outside the regular line of professional advancement, and when eventually he was recognized by fellow psychiatrists in Zurich, the main direction of his thought had been definitely and firmly set. He had even acquired a group of disciples in Vienna and elsewhere, and the time had passed, if indeed it had ever existed, when Freud could be absorbed by a professional group as a member on an equal footing with other members.

His capacity for maintaining a course of action without the support and encouragement of his profession was an essential factor in his achievement. Whatever else his teachings may be, they are those of a man working at his own task in his own way. Nothing in his career is more remarkable than the complete trust he placed in his own perceptions. As one reads his comments on those who did not wholly agree with him, one is struck by the fact that it apparently did not occur to him that divergences from his own opinion might be anything but errors [2]—errors due to an inability, temperamental or otherwise, to understand and accept what he so plainly saw to be true. In marked contrast to his repeatedly demonstrated readiness to change his theories when his own observations and reflections required it, was his resentment of changes suggested by others, especially those who had once been his followers. Apparently he could accept the contributions of disciples while they remained disciples; but the give and take between colleagues on terms of equality seems to have been foreign to his nature.

Without his independence of professional support and professional conventions, Freud could not have accomplished his task. Without it, he could never have maintained the novel and difficult direction of attention which gave his contribu-

[2] There are illustrations of this attitude scattered throughout Freud's works. They are especially numerous in his History of the psychoanalytic movement (translated by A. A. Brill). *Nerv. & Ment. Dis. Monogr. Series*, No. 25, 1916, 1–58.

tions their distinctive character. For the uniqueness of his achievement was determined by the fact that he saw human beings neither as common sense saw them nor as they appeared to the eyes of any existing profession. He addressed himself to problems of human nature which were not quite the problems anyone else had seen; his interest was attracted by a field of observation not exactly that which anyone else had explored or even clearly noted; and he gradually evolved a conceptual system which, derived from his personal observation in his special field, came nowhere near engaging smoothly with the accepted conceptions of any professional group. In a sense, of course, Freud was working on an age-old problem, that of inner conflict. In a narrower and more professional sense he was observing the material every psychiatrist must perforce observe. But his attention was caught by occurrences and connections no one else had made focal and steadily regarded, and in reflecting on them he made explicit and welded into a connected (though not closely articulated) system, facts and relationships which others had overlooked or had only dimly sensed or had noted and reported as fragmentary observations.

Perhaps Freud's most striking intellectual characteristic is his utter absorption in the class of facts he had set himself to understand. At any rate it was this absorption, unhampered as well as unsupported by professional tradition, that gave his theory both its strangeness and its scope—on the one hand, putting it out of line with the intellectual mores of the day, and on the other, by keeping it close to the concrete and the actual, making it relevant to a wide range of human interests. Freud's disregard of the accepted rules of science and scholarship is notorious. Using the special tools of no profession or craft, he had recourse to a method more akin to common observation and common sense than to the specialized techniques of any of the established intellectual disciplines. Fundamentally his observations were like those involved in the social perceptions and judgments of everyday life—more sensitive and more shrewd, of course, and inevitably marked by his personal preoccupations—but dependent

nevertheless on the ordinary operations of social intelligence. It is interesting that his special method uses the commonest mode of social communication. Psychoanalysis is carried on by means of talk, conversation. Indeed, much of the strength of Freud's theories and practices lies in the fact that his methods were *un*technical and *un*specialized, and therefore kept him close to the familiar actualities of common life and common sense.

Yet is would be manifestly absurd to identify Freud's theories with the insights of common sense. Rather they were the outcome of explorations into the subsoil of common sense; and if for this reason they are relevant to the whole range of human endeavor, they are for the same reason difficult to place with reference to existing bodies of knowledge and modes of procedure, not excluding those of common sense. It is extraordinarily difficult to describe Freud's method in any but negative terms. Perhaps the nearest approach to a positive description may be found in Emerson's phrase 'man thinking.' 'Thinking in unhabitual ways' is another phrase that suggests itself as appropriate, especially when it is recalled that Freud's inquiries have been directed toward the very facts concerning which conventional modes of thought have been most subject to the pressure of habit and custom. Freud's task required the difficult and delicate adjustment of focusing just off the usual fixation point; of maintaining a line of regard almost coinciding with, yet always distinct from, the one favored by habit and common practice. Furthermore his observations were a personal enterprise, one in which he neither utilized, nor indeed felt the need for, the safeguards which the sciences find indispensable.

No psychologist need be told that this mode of observation was not one approved by the psychology of Freud's day. It is a commonplace that when psychology emerged as a science, its goal and direction, along with its method and assumptions, were determined by the physical sciences of the nineteenth century. No more impersonal psychology has ever existed than that which developed in the first psychological laboratories. There is a sense in which its special method, intro-

spection, represents the height of objectivity and detachment, requiring as it does the observation of events defined as available only in immediate experience, with the same disregard for personal values and implications demanded of an experimenter observing any event, say a falling body, in the external world. The introspection of the early laboratories was not self-exploration and insofar as psychoanalysis was precisely self-exploration for the person analysed, it ran directly counter to the approved methods of the psychology of the time. It would be difficult to find a task in sharper contrast with that of the 'observer' in a psychological laboratory than that required of a patient undergoing psychoanalysis.

This difference in method and outlook becomes especially significant when one considers how closely Freud's career coincides in time with the rise and development of psychology as an experimental science. Freud was born in 1856, four years before the publication of Fechner's *Elements of Psychophysics*, the book commonly regarded as the first achievement of a definitely experimental psychology. By the time he had reached university age, the early classics in psychology had been written. During his own student days at Vienna, Wundt's famous laboratory was opened at Leipzig, and in the decade of the eighties—the formative period of his professional life—such notable pioneer studies in psychology were appearing as Galton's researches on imagery and association, Ebbinghaus' monograph on memory, and the contributions of James and Lange to the theory of the emotions now known by their names. The list might be extended but it has gone far enough to exhibit in an account of Freud's work the character of 'not belonging.' Yet imagery and association, memory and emotion, are processes of vital importance to the Freudian theories and to the success of psychoanalysis as therapy.

The fact is that Freud's interests were so completely outside the psychology pursued by professional psychologists that he was altogether unaffected by their activities. To be sure, there was a time when he acknowledged the usefulness of the free association experiment which Jung introduced from the

psychological laboratory. But in his *History of the Psychoanalytic Movement*,[3] written after the break with Jung, his comment on this importation is entirely unenthusiastic and constitutes the only reference to experimental psychology in the entire account. It is interesting too that the five lectures entitled *The Origin and Development of Psychoanalysis*,[4] given at Clark University at the invitation of G. Stanley Hall, likewise contain only one reference to a psychological study; a reference, moreover, which seems an expression of courtesy rather than of an interest in the findings of psychologists. Freud's indifference, furthermore, persisted throughout his career. It remained completely unaltered when psychology itself changed, partly by enlarging its field and including problems similar to his own. The mere suggestion that his attitude might have been otherwise is slightly absurd. It is interesting, nevertheless, as additional evidence of his extraordinary absorption, that a whole new intellectual movement arose and developed in a field presumably related to his own, without arousing his curiosity or even attracting his serious attention.

It is interesting, too, that psychology did not meet indifference with indifference, but paid far more heed to Freud than to most of its own workers. Why then did Freud, who had no interest in psychology, become one of the most potent influences in the psychology of his day?

An obvious answer, and one that is less trivial than it seems, is that psychologists would have been blind and deaf to what was going on in the world about them if they had not noticed the tremendous stir Freud was creating concerning topics presumably within their own field.

Many psychologists, of course, are almost that. In other words, many of them are almost as absorbed in their special problems as Freud was in his, and some of the problems are

[3] *Op. cit.*, p. 20.

[4] S. Freud. The origin and development of psychoanalysis (translated by H. W. Chase, revised by Freud). *Amer. J. Psychol.*, 1910, **21**, 181–218. The reference to a psychological investigation occurs on p. 208. The study referred to is 'A Preliminary Study of the Emotion of Love between the Sexes,' by Sanford Bell, which appeared in the *Amer. J. Psychol.*, 1902, **13**, 325–354.

such that they can be profitably investigated without reference to the teachings of Freud. It is well to recognize that this is the case. If psychology is defined as the work psychologists are doing, it becomes a mere matter of empirical fact that there are portions of psychology which have been as unaffected by Freud, as Freud's teachings have been unaffected by psychology.[5] This means, among other things, that the subject matter of psychology is not limited to problems of personal adjustment. It is well that this point be explicitly stated since it is one that most psychoanalysts and some psychologists seem to find either inconceivable or incredible. Yet in the *difference* between the tasks of psychologists and psychoanalysts lies the reason for their different attitudes toward each other. Psychoanalysts, whose activities and thoughts are centered about a special problem which is both practical and pressing, can afford to work at this problem with little concern for what most psychologists are doing. Psychologists, however, undertaking a more detached but more comprehensive survey of human nature, find it impossible not to listen to Freud. Even the most specialized workers find it hard to be unaware of the total field of which their problems are a part. They can therefore be only 'almost' as absorbed in a special problem as Freud. Psychologists know too that the field Freud explored is one in which their own efforts have been most fruitless. It is interesting to find a symposium on Freud's concepts which includes among its participants a number of psychologists whose chief contributions and characteristic interests are elsewhere.[6] Psychologists do not find it possible to ignore Freud and his theories, as Freud and his followers have found it possible to ignore psychology. Psychology, aiming at comprehensiveness, is aware of incompleteness.

A sense of the incompleteness of psychology, however, does not explain the positive appeal of Freud's teachings. Not everything that promises to deepen and vitalize psycho-

[5] Any issue of the *Psychological Abstracts* may be referred to as evidence on this point.

[6] Contributions of Freudism to psychology. PSYCHOL. REV., 1924, 21, 175–218. The participants are L. L. Thurstone, J. H. Leuba, K. S. Lashley, J. Jastrow.

logical knowledge[7] gains from psychologists the respectful hearing they have given this theory based on methods and conceptions so different from their own.

There is no escaping the fact that the Freudian theories found their way into psychology without the backing of accepted, or acceptable, scientific evidence. They entered with no support but that of common knowledge about human nature, knowledge neither more nor less accessible to psychologists than to anyone else. There is a sense in which Freud's teachings were unfamiliar to few, in which despite their boldness and originality, they were often heard even at first with something like recognition. To be sure, Freud's theories were considered bizarre. No one had seen exactly what Freud had seen and no one had seen all that Freud had seen; yet many had noticed something sufficiently similar to arouse their willingness and eagerness to listen to his interpretations. It is this obscure relevance to common knowledge that made Freud's teachings credible, though not always acceptable, to the man in the street; and it is in his role as man in the street, not on the basis of his special knowledge, that a psychologist pays attention to Freud.

But more than vague relevance to common knowledge was needed to give Freud's theories the importance they attained. An essential part of Freud's contribution is the form in which he presented his teachings, a form which made statable for open and public discussion events which occurred in hidden private worlds. In brief he invented a mythology and a terminology. By the liberal use of analogy and metaphor he constructed a world of symbols well adapted to the human propensity for thinking in terms of concrete situations: a world not of abstractions, difficult to conceive and attend to, but of picturable persons and objects and places, as easy to think and talk about as the world of a novel or drama, and somewhat similar in its appeal to human interest. By reference to this world layman and scientist alike found it possible to formulate the problems of depth psychology. It is

[7] An interesting comparison, one which would reveal some of the basic assumptions of psychology, might be made between the attitudes of psychology toward psychoanalysis and toward E. S. P.

profoundly significant that the Freudian terminology has been widely adopted, and that even among psychologists who find Freud's explanations worthless, there are many who find his terminology indispensable. Whether the terminology would have become current without the mythology is a question. The mingling of the two is now a *fait accompli*, and a source both of strength and of confusion in the Freudian system.

It is a source of strength because, as has just been suggested, it takes advantage of the mode of thought which human beings find easiest and most effective. By using a terminology which externalizes inner conflicts, by presenting them imaginatively as if they were situations like those encountered in the external world, Freud did much toward placing the emotional entanglements of human beings in a context in which they could be looked at objectively and recognized as problems for scientific investigation. The confusion arises from the fact that the device is not only helpful but dangerous, offering as it does an ever present temptation to reification. Imagined objects and situations may all too readily be thought of as actually existing. Furthermore, when used as symbols, they involve the risk of surrounding the concept with irrelevant implications. They place before the reader more than is there in order to make him see that something is there. The question then rises, Can the excess, which is merely arbitrary and may therefore be misleading, be effectually cleared away or at least rendered harmless? Can products of the imagination be transformed into scientific concepts?

Murray's *Explorations in Personality*[8] is an attempt to come to terms with this problem by one whose main interests are in clinical rather than in academic psychology. But academic psychologists are concerned with a problem similar in general outline. Among these are men of such different interests and academic backgrounds as Tolman, Lewin, and G. W. Allport—all far more impressed than Freud by the requirements of science, but all finding the conventional

[8] H. A. Murray. *Explorations in personality.* New York, London: Oxford University Press, 1938.

systems of psychology inadequate to the problems with which they deal. Thus from within academic psychology itself there has come a demand for concepts suitable to dynamic problems. Perhaps not the least of Freud's achievements is the impetus he has given to a movement in which he seems to have had no interest whatsoever, a movement toward a revision of the theoretical framework of academic psychology for the accommodation of problems in dynamics.

But while gaining attention for some aspects of human nature, Freud needlessly thrust others into the background. Absorbed in the psychology of the primal urges, he paid little attention to the implications of the fact that the human animal is capable of intelligent action. Yet one of the conspicuous differences between human beings and other animals is the greater extent to which the human species employs the mode of adaptation called intelligence. No other species develops arts, sciences, and philosophies; no other species produces and destroys civilizations. To ignore such conspicuous differentiæ of a species is as one sided and unrealistic as to ignore those characteristics which place it in the same class with other animals.

Of course Freud did not ignore the intellectual processes completely, but in comparison with the non-rational urges they seemed to him unimportant. It is significant that he never worked out a very clear position with respect to the intellectual processes. At first he rather vaguely regarded them as associated with the ego, admitting that there was much in the ego that he did not understand. Thus he conceived of the ego as more or less effectively opposing the libido, and at the same time he thought of the intellectual processes as subservient to the libido. Rationalization and autistic thinking he regarded as typical intellectual activities. It is unfortunate and somewhat strange that the intellectual operations as such did not arouse his curiosity, for the very occurrence of such practices as rationalization and day dreaming suggests that intellectual activities are not incapable of imposing their own conditions. Does not rationalization suggest that there are intellectual demands which must be satisfied, even when

the main object of the intellectual activity is to serve the non-rational impulses? And is not some explanation required for the fact that imperious primal urges can be satisfied, even temporarily and partially, by such flimsy stuff as day dreams? At any rate a field of inquiry is indicated concerning lines of relationship between cognitive activities and the primal urges.

As a matter of fact, a recognition of something of the sort is implied in the direction Freud's own thought has taken. In his later theories he posited, largely as a result of a study of the ego trends, a far greater complexity of organization, a far more intricate system of interrelationships among the parts of the personality, than that pictured in the simple opposition of ego and libido. The old theory seemed inadequate to the complexities of a creature capable of going beyond the pleasure principle. In consequence Freud developed the concept of an elaborate organization consisting of the ego, super ego, and id. The new theory, while keeping the emphasis unmistakably on non-rational urges, nevertheless definitely recognized in the cognitive and intellectual processes distinctive and essential and complicating factors involved in the production of conflicts and in the formation of the personality. It also recognized strong alliances as well as bitter conflicts between the rational and non-rational components of human nature.

It is impossible, of course, to foresee the lasting outcome of Freud's system, but the general character of his contribution is easily discernible. Like Copernicus and Darwin—the comparison has become inevitable—he put the facts of common observation in a setting which profoundly altered their meaning, and which introduced into both science and common knowledge radically new perspectives. But unlike Copernicus and Darwin, Freud presented his theories in a form unsuitable to scientific verification and use. The situation abounds in difficulties and recalls the half despairing advice given by William James in another connection: "The only thing then is to use as much sagacity as you possess and to be as candid as you can."[9]

[9] W. James. *The principles of psychology*, Vol. I. New York: Henry Holt and Co., 1890, p. 92.

A SYSTEMATIC APPLICATION OF THE CONCEPTS OF GENERALIZATION AND DIFFERENTIATION TO VERBAL LEARNING[1]

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Introduction

A recent trend in experimental and theoretical psychology has been the attempt to systematize the facts of a given field on the basis of empirical principles derived from study of the conditioned response (**14**, **15**, **21**, **39**, **16**). It is reasonable to expect that the same general laws hold from one learning task to another, in so far as the situations are similar, and it is possible that the greater the simplicity of the learning, the better are the chances of laying bare mechanisms which operate in all learning.

It is necessary, however, to guard against the chance that the principles chosen for systematic exploitation are artifacts of the experimental situation from which they were derived, and have no importance as general characteristics of learning. Hull has suggested a procedure for testing the value of a theory built on such concepts (**14**). The essentials of such a test are the development of a clear-cut hypothesis, exploration of the hypothesis for all its derivatives, and experimental test of such derivatives as have received no direct substantiation or disqualification. The theory must stand or fall with the experimental evidence for or against the predictions which it makes.

The following account is an attempt to develop a theory relating various facts of verbal learning (such as transfer, interference, certain intra-list effects and retroactive inhibition), to two experimentally defined characteristics of the con-

[1] This article is a part of a dissertation presented to the Faculty of Yale University in partial fulfillment of the requirements for the Ph.D. degree. The writer wishes to express thanks to Professor Clark Hull for his advice and assistance.

ditioned response—generalization and differential inhibition. The theory will be explored for deductions of known facts as well as for implications yet to be tested, since it has been undertaken principally in the hope of systematizing many of the facts already known. That transfer, interference, and retroactive inhibition are related has long been suspected, as the so-called 'transfer' theory of retroactive inhibition implies (2, p. 427 ff.). The hypothesis to be presented is likewise classifiable as a 'transfer' theory, but it aims to make as explicit as possible the mechanisms assumed to be responsible for the transfer, which is itself a phenomenon as poorly understood as retroactive inhibition.

The hypothesis will first be stated in a brief, informal fashion. Then, for the benefit of the reader who wishes to make a more scrupulous examination, the terms used will be explained, assumptions stated, and arguments for various predictions presented in detail.

Statement of the Hypothesis

The hypothesis asserts that a major necessity of verbal learning is the establishment of *discrimination* among the items to be learned, and that this process of discriminating is actually a fundamental part of what is called generally the learning process. If no discrimination between the items already exists, then the early part of the learning process will see an *increase* in the tendency to confuse the items, followed by the development of discrimination. Learning time should be at a maximum in this case. If such discrimination already exists, learning time should be at a minimum. Positive transfer will occur in situations where the nature of a second task permits discrimination acquired in a previous task to be beneficial. Negative transfer will occur when generalization with a previous task occurs, but where the situation is such that discrimination *between some aspect of the two tasks themselves* is required, as well as learning of the second. Retroactive inhibition will occur, similarly, if a second task generalizes with one already learned, and if the situation is such that discrimination between some aspect of the two tasks must be produced before the first can be recalled adequately.

The reader familiar with the facts of conditioned responses will see immediately the possibility of stating these assumptions in terms of *generalization* and *differentiation*.[2] The hypothesis will be stated as it applies to verbal learning by the paired associates method. It is assumed that when a list is being learned by this method, generalization may occur between the various stimulus items, so that a response learned to one tends to occur as a response to other stimulus items in the list also. Figure 1 represents a list in which generalization is occurring. The dotted lines represent generalization tendencies and the solid arrows represent connections with the "right" responses, which are to be learned. In such a list as this, where the generalizing stimulus items all have

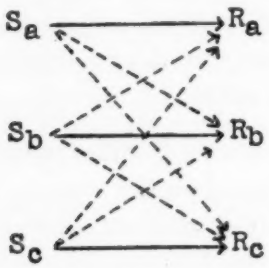

FIG. 1

different responses, those responses occurring or tending to occur by virtue of generalization will block the right responses in proportion to the strength of the tendency to generalization. In order to reduce the strength of the generalizing tendencies (to increase the differentiation), differential reinforcement through practice must be applied. It is assumed that varying degrees of generalization may occur between one item and others in a list, and consequently that lists may vary as to the average strength of generalization occurring in the list. The necessity for greater differentiation will then serve to make the learning more difficult in lists where a high degree of generalization occurs than in ones where less generalization

[2] The term *differentiation* will be used in place of *differential inhibition*, since the term 'inhibition' implies a theory of differentiation which the present hypothesis need not assume.

occurs; and it will also be responsible for poorer retention, since the differentiation, especially with lower degrees of learning, decreases over a period of time, allowing spontaneous recovery of the generalization. If the above hypothesis is correct, the generalization in either case should be apparent in actual confusion of responses.

Furthermore, in any two-list situations such as the ones used to study interference and retroactive inhibition, it is assumed that inter-list generalization may occur. When a first list is followed by a second one which includes stimulus items similar to ones in the first, stimulus items in the second list will tend to produce responses from the first; and likewise when list 1 is again presented for recall and relearning, response members from list 2 will tend to recur. Figure 2

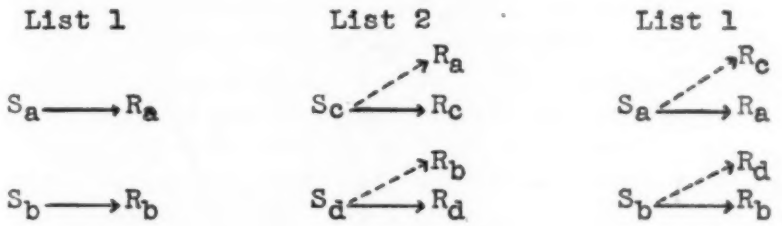
List 1
List 2
List 1
$S_a \longrightarrow R_a$
$S_b \longrightarrow R_b$
$S_c \longrightarrow R_c$ (dashed to R_a)
$S_d \longrightarrow R_d$ (dashed to R_b)
$S_a \longrightarrow R_a$ (dashed to R_c)
$S_b \longrightarrow R_b$ (dashed to R_d)

FIG. 2

represents responses from list 1 tending to occur by generalization in list 2, and ones from list 2 occurring by generalization when list 1 is again tested. In each case, the generalizing tendencies will block the right excitatory tendencies in proportion to the strength of the generalization. Again, the generalization should be evident in actual confusion of responses—in this case, errors of 'reversion' to the wrong list.

The degree of learning of any list cannot be predicted from the number of practice repetitions alone, according to this hypothesis, since the amount of differential reinforcement or practice required to reduce generalization to a given strength increases with the *degree* of generalization between stimulus items. The extent of differentiation achieved as a result of a given amount of differential reinforcement will vary,

then, with this factor. Moreover, the extent of differentiation must be calculated also in terms of time elapsing since differential reinforcement has been discontinued, since spontaneous recovery will eventually occur. Pavlov has shown that spontaneous recovery occurs more slowly, the farther differential inhibition has been carried (**33**, p. 123). Temporal intervals between learning and recall thus assume importance in the light of the hypothesis, and together with degree of learning, must be taken into account in predicting inter-list effects.

The hypothesis has been stated in terms of lists learned by the method of paired associates, but the same logic will apply throughout to lists learned by any method which allows a stimulus-response analysis. Figure 3 represents a list being

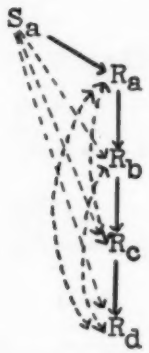

FIG. 3

learned by the *anticipation* method. Item S_a functions only as a stimulus and must produce the next item, R_a, as its response. But when this item actually appears, it functions as a stimulus. The dotted lines represent generalizing tendencies within the list. The hypothesis cannot be so easily applied to other verbal learning situations, since in most of them the subject's performance is not so controlled as to allow a detailed S–R analysis. Such would be the case with the method of complete presentation, for instance. However, there seems no good reason to suppose that the hypothesis *cannot* hold with such a method. In the sections which follow, evidence will be quoted without comment when it has

been obtained by the paired associates or anticipation method, but where any other method has been employed, this fact will be specifically referred to.

The role of the *responses* in determining ease of learning a single list or in transfer situations is not specifically delineated by the present hypothesis. It is conceivable that something analogous to generalization of stimulus items might occur among response items, especially since every response is in a sense a stimulus as well. Müller's substitution hypothesis develops this possibility (**30**). According to Müller, 'active' substitution was said to occur when idea *a* was connected with idea *b*, and idea *A*, which was similar to *a*, also produced *b*. This description follows the pattern of sensory generalization which has been described here. But Müller also described 'passive substitution'; *B* might be substituted for *b*, when *B* was similar to *b* but not associated with *a*. This, in terms of the present hypothesis, would amount to generalization of response items, if it occurred. Confusion errors would result in either case, and *a priori* might be due to either type of generalization. But recent work of Thorndike (**41**) suggests that the stimulus items of a list are chiefly effective in producing such errors. He found that stimulus members of a very long list evoked responses made to *other stimuli like them* nearly twice as often as they evoked responses connected with unlike stimuli. But a stimulus member evoked a word which was *like the response* connected with it little or no oftener than it evoked unlike responses.

An experiment by McGeoch and McGeoch (**28**) seems also significant in this connection. In a retroactive inhibition experiment, stimulus members of corresponding paired associates in a list 1 and a list 2 were synonymous; or response members were synonymous; or both members were synonymous; or neither were. When the stimulus members were synonymous, there was a consistent increase in retroactive inhibition over the condition where neither member was synonymous, but this was not the case when response members were synonymous. Furthermore, when both members were synonymous, there was no consistent increase in retroactive

inhibition as compared with the condition where stimulus members only were synonymous. On the basis of these results, the present hypothesis will confine itself to the exploitation of the concept of sensory generalization as originally stated.

Relation to Other Systems

There has been in the past at least one rather ambitious attempt to systematize some of the facts which are being considered, and there are contemporary theories which apply to the same realm. The earlier attempt referred to is that of G. E. Müller and his students. Müller and Pilzecker (**30**), besides presenting an enormous mass of experimental data based on a thorough-going exploitation of the *Treffermethode*, posited a number of kinds of inhibition (*e.g.*, associative inhibition, retroactive inhibition) to explain their results. Their substitution hypothesis, which rose from an exhaustive analysis of errors, in a sense foreshadows the present theory. Müller's theories, naturally, rose out of his results; and though his terminology has left a lasting impression on the literature, it seems antiquated today, because the foundation laid by these earlier investigations makes it possible to develop a more comprehensive theory which may even suggest the psychological processes or mechanisms underlying his types of inhibition.

A systematic attempt to explain various facts of memory and learning is being made at present by Gestalt psychologists, and has given rise to experimental work, notably by Köhler and his students (**20**, **31**, **32**, **35**). The essence of the theory is that the Gestalt laws of spatial organization hold also in the field of memory; by analogy with their laws of perceptual organization, the Gestalt psychologists predict such effects as difficulty in learning homogeneous series, retroactive inhibition, and proactive inhibition. For instance, the 'law of similarity in spatial organization' is assumed to hold for memory, and is demonstrated in a series of experiments by von Restorff (**35**), in which retention of homogeneous series of items is compared with retention of heterogeneous series.

An item in a heterogeneous series is said to be in a better position to be retained than the same item in an entirely homogeneous series, since by the law of similarity there is *aggregation* of the traces of the homogeneous items, thereby causing any single item to lose its identity. Also, an 'isolated' item in one series may lose its superiority if a second series follows which is composed of items similar to it (retroactive inhibition), while an isolated item in a second series will have no advantage if the series preceding it is made up of similar items (pro-active inhibition). The theory is characterized by Koffka as a 'dynamic trace theory' and has been elaborated in his book (**19**). It could probably be restated in terms of discrimination, and in this sense is similar to the theory presented in this paper. The chief difference lies in the concepts on which the two theories are based, and the methods by which they are developed.

A third theory, advanced by James (**17**) to explain interference, rests like the present theory on Pavlovian concepts. His theory differs considerably from the present one, however, in that the key concept is external inhibition, rather than differential inhibition. The theory is extended to cover retroactive inhibition, but it is unfortunately not developed to the extent of predicting experimental results or specific tests. No experimental work seems to have followed it. The theory could consistently supplement the one suggested in this paper.

The present-day associationists have assembled a number of 'laws' which bear some relation to the central notions of the present hypothesis. In Robinson's book (**36**), a *Law of Assimilation* is included which is very similar to the notion of generalization. It is stated as follows: "Whenever an associative connection is so established that an activity, *A*, becomes capable of instigating an activity, *B*, activities other than *A* also undergo an increase or decrease in their capacity to instigate *B*." The law is extended to cover 'assimilation of instigated processes' as well. The *Law of Acquaintance*, suggested in the same work, is superficially similar to a principle of discrimination. But the law apparently means by acquaintance mere isolated repetition of a given item; 'ac-

quaintance' is relevant to the present theory only in so far as it produces discrimination from other items. Mere acquaintance does not seem to increase efficiency of learning (Waters, **42**). Much closer to the notion of discriminability of stimulus items is Thorndike's *Law of Identifiability*, which states that "connections are easy to form in proportion as the situation is identifiable, distinguishable from others . . ." (**40**, p. 87). In general, these laws seem to name results, rather than to form a coherent and interrelated system.

Explanation of Terms

The following terms are crucial to the hypothesis and are consequently explained in some detail. Illustrations will be presented when necessary.

1. *Generalization:* the tendency for a response R_a learned to S_a to occur when S_b (with which it has not been previously associated) is presented.[3] A *generalization gradient* is said to be formed when a number of stimulus items show decreasing degrees of generalization with a given standard stimulus. The hypothesis need make no assumption as to the type of stimulus continuum which will yield a generalization gradient, but it is consistent with it to suppose that such a gradient will be yielded by a group of stimuli which can be arranged along *any dimension or scale* with respect to the presence of some discriminable quality or aspect—in other words, stimuli which would be considered to vary in degree of *similarity*. Experimental evidence proves that not only simple dimensions such as pitch and intensity (**11**, **12**) yield generalization gradients. Yum (**44**) found that when nonsense syllables, words, or visual patterns were stimuli in a learning series, different but similar stimulus items would in a test series elicit the responses learned, the strength of the tendency varying with the degree of similarity between test item and original stimulus item; and Gulliksen (**10**) found that similarity of test figures to a

[3] It will be noted that this definition makes no reference to nervous processes or to any physiological explanation. This usage is in accordance with that followed by Bass and Hull (1) and Hull (16). That generalization will occur with voluntary verbal responses in the form of a typical generalization gradient has been demonstrated by Gibson (6).

training figure correlated sginificantly with tendency to give the training figure's response. Razran (**34**) has recently demonstrated generalization on the basis of synonymity of words.

2. *Multiple generalization:* the tendency for responses which are being learned to members of a *series* of stimulus items to occur as generalized responses to members other than their 'right' associate. The situation employed in Yum's experiment (**44**) was such as to yield multiple generalization. Gulliksen's experiment (**10**) provides a transition between simple generalization and multiple generalization, since a choice between two antagonistic responses, each of which had been learned to a standard stimulus, was possible.

3. *Right response:* the response paired with a particular stimulus item in a list presented by the paired associates method, or the response immediately following a particular stimulus item in a list presented by the anticipation method.

4. *Learning:* a list is said to be completely learned when the right response item is given in each case upon presentation of the stimulus item. A given *degree of learning* or criterion of learning means that a certain percentage of right responses is reproduced upon presentation of the stimulus items.

5. *Differentiation:* a progressive decrease in generalization as a result of reinforced practice with $S_a \rightarrow R_a$ and unreinforced presentation of S_b.

6. *Reinforcement:* a process which occurs during verbal learning when a subject sees a response as he anticipated it and thinks "that's right." Differential reinforcement designates the situation wherein a right response is reinforced and a generalized response is not reinforced.

7. *Excitatory tendency:* tendency for a particular stimulus to evoke a particular response in a degree greater than zero.

8. *Meaning:* a characteristic of a verbal or visual item which serves to differentiate it from other items.

Statement of Postulates

A number of postulates are basic to the hypothesis and should be made evident at the outset. In some cases these principles have been empirically demonstrated in the condi-

tioned response situation; in other cases they are incapable of empirical demonstration, and must rest on confirmation of the relationships which they predict in combination.

1. When lists are learned by a paired associates or anticipation method, *S-R* connections are set up between certain of the items (between members of a pair in paired associates, and one and the next in the anticipation method).

2. If there is a right excitatory tendency and also a generalized one between the same *S* and *R*, the resultant excitatory tendency will be stronger than either one alone; if a right excitatory tendency and a generalized one are aroused by the same *S* but lead to different *R*'s, they will interfere, the weaker tending to block the stronger in proportion to the strength of the weaker.

3. Stimulus items which generalize when presented for learning in a single list will also do so when they are presented in the form of two lists, and with the same *relative* degree of generalization.

4. Generalization will increase to a maximum or peak during the early stages of practice with a list, after which it will decrease as practice is continued. (This assumption has been confirmed by the author in an experiment to be reported, **7**.)

5. In list-learning, all decrease in generalization is due to differential reinforcement.[4]

6. The amount of reinforcement required to reduce a generalizing tendency to a given strength will increase with increasing strength of the generalizing tendency. (This postulate is an 'empirical' one, in the sense that it has often been demonstrated in the conditioned response situation, **33**.)

7. After the cessation of practice, differentiation will decrease over a period of time, leading to an increase ('spontaneous recovery') of generalization. (This, again, is an 'empirical' postulate, **13**, **33**.)

[4] There is a possibility that a slight increase in specificity might be obtained by pure practice of a single connection. This possibility has not been included in the present system because the great overelaboration necessary would be out of proportion to the importance of the possibility.

8. Spontaneous recovery will occur more slowly, the more differential reinforcement has been applied. (Demonstrated in the conditioned response situation, **33**.)

9. If differentiation has been set up among a number of stimulus items, it will be easier to differentiate them again later, even though they are paired with different overt responses than those learned when the original differentiation was set up. (This assumption might be called 'transfer of differentiation.')

10. If differentiation has been set up among a number of stimulus items, there will be less tendency for them to generalize with new stimulus items or for new stimulus items to generalize with them, the decrease in generalization being proportional to the amount of differential reinforcement given. (Analogous to this are the cases reported in Pavlov (**33**), of dogs who, having once learned to differentiate a circle from an ellipse, found it easier to differentiate between the circle and other ellipses of different ratio. A common sense analogy would be the case of a child who, after at first generalizing all the animals he sees, learns that cats are not dogs; after this, he does not have equal trouble in learning that rabbits are not dogs. He would not have to see every animal in the world in order to differentiate dogs from other animals.)

Some Propositions that Follow from the Hypothesis

A number of propositions which may be predicted from the hypothesis just presented will be stated. These will not be formally derived in the strictest logical sense, but for each one the argument will be indicated as clearly as possible, with reference to the definitions and postulates on which it depends. Examples will follow the argument when any clarification seems necessary, and evidence for or against the proposition will be reported. In some cases, the propositions are familiar facts of verbal learning. For the convenience of the reader, the propositions will be given titles and grouped under traditional headings.

Similarity

Similarity will be considered, in the present instance, as that relationship between stimulus items which can be indicated and measured in terms of their *tendency to generalize.* A priori, this is a reasonable criterion of similarity, since a group of stimuli among which generalization occurs would, according to usual standards, be considered similar (*e.g.*, a series of tones, or of spots along the fore-leg). Yum (**44**) found that judges' ratings of perceptual similarity of visual patterns and synonyms to their respective standards coincided with the degree to which the forms or synonyms were capable of producing a response originally learned to the standard. Furthermore, it seems obvious that a common way of speaking of similarity is in terms of discriminability, or tendency *not* to generalize.

A complication in terminology is introduced by the fact that generalization will vary with other factors than 'original' generalization tendency. As has already been pointed out, it will vary also with degree of learning and with the time interval since learning was left off. These two factors are in a sense secondary determiners of degree of generalization, and "original generalization tendency" will be used therefore to denote the degree of generalization apparent before differentiation has been set up through differential reinforcement. Under the heading of "similarity," then, will be included propositions which have as their major variable the degree of original generalization of stimulus items.

I. *Intra-List Transfer and Concept Formation*

If multiple generalization occurs during the learning of a list, and if the list is constituted so that the generalizing stimulus items are paired with the same responses, learning should be easier in proportion to the degree of generalization.

The argument for this proposition would be as follows: Suppose that a list is to be learned in which multiple generalization may occur, and suppose that those stimulus items which are similar (*i.e.*, generalize to a high degree) actually have the *same* responses to be learned to them. The generalizing excitatory tendency and the

right excitatory tendency, in this case, will coincide. According to postulate 2, if there is a right excitatory tendency and a generalized one between the same S and R, the resultant excitatory tendency will be stronger than either one alone. Now, according to the definition of generalization, generalization may exist in different degrees, so that the aggregate generalization in a list may vary from low to high. Then, in the present case, the higher the degree of generalization in the list, the stronger will be the resultant excitatory tendencies after the same amount of practice, and the easier will be the learning.

An example of such a list is presented in Fig. 4. As the broken

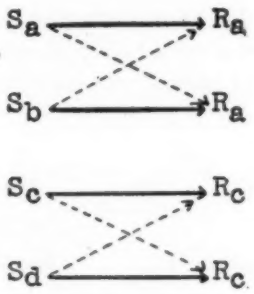

FIG. 4

arrows indicate, items S_a and S_b generalize, and items S_c and S_d generalize; and both items of a pair have the same response. The higher the degree of generalization between the groups of items having the same response, the easier the list will be to learn. The important thing about this situation is that the arrangement is the one commonly employed in studying concept formation, which suggests that the process called concept formation is related to the process of verbal learning, since both depend upon differentiation and generalization. Since control lists where little generalization occurs are not usually included in concept experiments, the above prediction has not been checked.

II. *Inter-List Transfer*

If generalization occurs from a first list to a second list, and if the generalizing items have the same responses, list 2 will be easier to learn as the degree of generalization between the two lists increases.

Suppose that a list 1 of paired associates is to be followed by a list 2 so constituted that generalization occurs from list 1 to list 2.

It is assumed (postulate 3) that stimulus items which generalize when presented for learning in a single list will also so do when they are presented in the form of two lists, and with the same relative degree of generalization. Now suppose further that the generalizing items in the two lists have the same responses. The reasoning given for the above proposition will then apply, and the greater the degree of generalization, the easier list 2 will be to learn.

An example of this situation, obviously a transfer one, is given in Fig. 5. The broken arrows in list 2 represent generalizing tendencies from list 1. But since the generalizing and the right responses coincide, positive transfer should result. It must not be forgotten that internal generalization will also occur to some extent in both lists; but it should not interfere with the transfer, since a control group learning merely list 2 would have the same internal generalization to contend with, and would only lack the beneficial generalization from list 1.

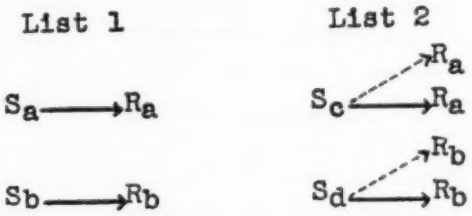

FIG. 5

No evidence obtained under these precise conditions is available, but an experiment of Bruce's (3) is relevant. He found positive transfer when the responses of two lists were the same, and the stimulus items very similar (two letters in common). Less transfer occurred when stimulus items had no letters in common.

III. *Retroactive Facilitation*

If generalization occurs between two lists, and if the generalizing items have the same responses, recall or relearning of list 1 will be facilitated as the degree of generalization between the two lists increases.

The situation is essentially that described in the above proposition, except that retention of list 1 is to be tested following the learning of list 2. The argument above will apply.

This situation might be dubbed *retroactive facilitation* (Fig. 6). Retention of list 1 should be facilitated increasingly as generalization

between the two lists increases, since learning list 2 should strengthen proportionally the excitatory tendencies already developed in learning list 1.

A retroaction experiment by Bunch and Winston (4) contained one condition in which response syllables from the first list were retained intact in the second, while the stimulus syllables were different. Inhibition, rather than facilitation, resulted during recall and relearning of list 1. This evidence is apparently not in harmony with the present theory. But degree of generalization of stimulus members was not varied or specified. A further test should be made where the stimuli are quantified according to generalizing tendency, so that several degrees of inter-list generalization may be employed. Relative degree of facilitation would be expected to follow the above prediction.

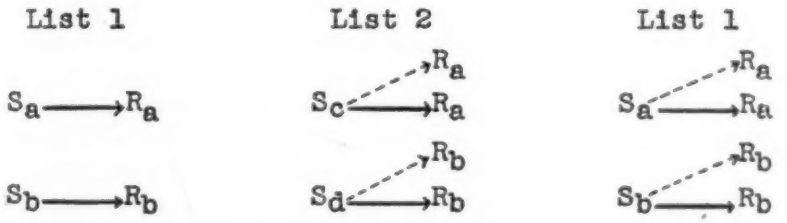

FIG. 6

IV. *Intra-List Interference or Difficulty of Learning Homogeneous Lists*

If multiple generalization occurs during the learning of a list, and if the list is constituted so that the generalizing items have different responses, an increasing number of repetitions will be required to reach a given criterion of learning as the strength of the generalizing tendencies increases.[5]

Suppose that a list in which multiple generalization may occur is so constituted that the generalizing stimulus items have different responses. The generalizing excitatory tendency, and the right excitatory tendency, in this situation, will conflict (see Fig. 1, p. 198). Two cases requiring slightly different arguments may result.

Case 1: If, in the above situation, the generalizing tendency is stronger than the right excitatory tendency after a few repetitions,

[5] From this point on, it will be assumed that the generalizing stimulus items have different responses, and the assumption will not be specifically mentioned.

it is obvious that the generalizing tendency will have to be reduced in strength, since learning is defined as giving the right response. But a decrease in generalization must be obtained through differential reinforcement (postulate 5), and the amount of reinforcement required to reduce any generalizing tendency to a given strength will increase with increasing strength of the generalizing tendency (postulate 6). Therefore, in this case, an increasing number of repetitions will be required to reach a criterion such as complete learning as the strength of the generalizing tendency increases.

Case 2: If, in the same situation, the right excitatory tendency is the stronger tendency after a few repetitions, it is nevertheless weakened in proportion to the strength of the generalizing tendency (postulate 2). By postulates 5 and 6, it is clear that an increasing number of repetitions will in this case also be required to reach a given criterion of learning, as the strength of the generalizing tendency increases.

In a list of the sort described, every right excitatory tendency will be interfered with more or less, according to either case 1 or case 2. Therefore, putting these two cases together, it may be said that an increasing number of repetitions of the list will be required as the strength of the generalizing tendencies within it increases.

This prediction is checked in detail in an experiment done by the writer (7), in which the lists compared are quantified according to degree of generalization. The evidence is conclusively positive. In so far as the generalizing tendencies are actually stronger than the right excitatory tendencies and above threshold strength, overt errors of confusion should occur during the learning. These did occur in the experiment reported, and with a much higher frequency in the higher-generalization series.

V. *Retention of Homogeneous Lists*

Difficulty in recalling or relearning a list will be proportional to the degree of original generalization, if time has been allowed for spontaneous recovery to occur.

In a situation similar to the one described above, the differentiation which was established during learning will decrease over a period of time, leading to "spontaneous recovery" of the generalization (postulate 7). The generalizing tendencies will again interfere with the right excitatory tendencies, in proportion to their strength (postulate 2), so that retention will be impaired in proportion to the degree of original generalization.

Positive evidence for this prediction may be found also in the writer's experiment (7). Retention of high-generalization lists was poorer than retention of low-generalization lists after one day, even though the former lists were given more than twice as many learning repetitions.

VI. *Learning of "Isolated" Items*

A stimulus-response pair which is a member of a list containing other stimulus items having a strong tendency to generalize with it will require more repetitions to be learned than would the same pair as a member of a list whose stimulus items have a low tendency to generalize with it.

This prediction is a special case covered by the argument under proposition IV. It has been stated separately, because the situation as put in the above prediction was the subject of an experiment by von Restorff (35). She presented to subjects series of paired associates which contained a high proportion of homogeneous items and a small proportion of items of other types. Items were always remembered better when they were 'isolated' than when they were members of a homogeneous group within the list; the list was given the same number of repetitions in the two cases. The prediction was confirmed in an experiment of the writer's (7) exactly as it is stated in this proposition.

VII. *Inter-List Interference as a Function of Similarity*

More repetitions will be required to learn a second list, in proportion to the strength of the tendency for items of a first list to generalize with the items of the second list.

Suppose that generalization occurs from a first list to a second list, as provided for in postulate 3, and as illustrated in Fig. 7. As these lists are constituted, the right response to any stimulus will conflict with any generalized response, since the responses are all

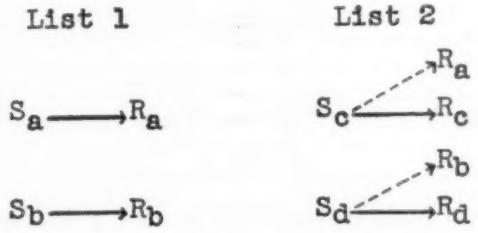

FIG. 7

different. Now, as list 2 is being learned, responses from list 1 will tend to occur to stimulus items in list 2 by generalization. According to postulate 2, the right excitatory tendency will be weakened in proportion to the strength of the generalizing tendencies. Generalization can be decreased only through differential reinforcement (postulate 5) and the amount of reinforcement required to reduce any generalizing tendency to a given strength will increase with increasing strength of the generalizing tendency (postulate 6). Therefore, as strength of generalization from the first list to the second list increases, more repetitions will be required to learn list 2.

Internal generalization will also occur as list 2 is being learned, but the prediction as made above will not be affected, since this factor may remain constant as generalization from list 1 to list 2 is increased. The situation described is the one known variously as proactive inhibition, interference, or negative transfer. Naturally, errors of reversion to list 1 should be apparent during the learning of list 2. An experiment done by the writer (8) demonstrated that list 2 required more trials to learn as degree of generalization between the stimulus items of the two lists increased. Also, the expected type of error occurred.

VIII. *Interference Measured by Recall*

A second list will be more poorly recalled after a period of time, in proportion to the strength of the tendency for items of list 1 to generalize with it.

Suppose that generalization occurs from a first list to a second list as in the above case, but that list 2 has been learned to some criterion (Fig. 8). It has been demonstrated in the above argument

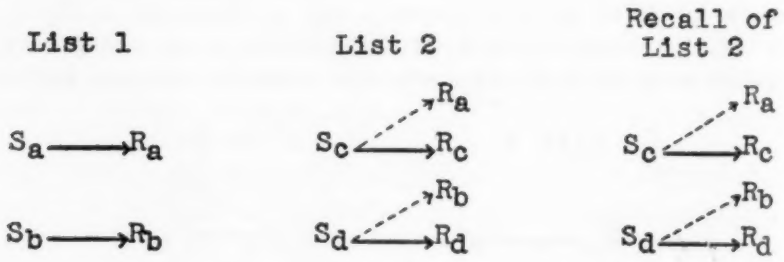

FIG. 8

that list 2 will require more trials to learn as generalization from list 1 to list 2 increases. As learning goes on, differentiation between

the items is established. But if an interval of time is allowed to elapse after practice has been discontinued, spontaneous recovery of the generalization will occur (postulate 7), so that recall should be impaired in proportion to the strength of the generalization from list 1 to list 2.

This prediction amounts to saying that interference as a function of generalization can be measured equally as well by a recall test of list 2 as by a learning measure. Whitely and Blankenship (43) used this measure to demonstrate interference in recall of a second list when the material learned consisted of monosyllabic words preceded in the first list by other such words or by nonsense syllables. The method of complete presentation was employed. Probably an even higher degree of interference would have been obtained had greater inter-list generalization been present (*e.g.*, had both lists been made up of nonsense syllables).

IX. *Retroactive Inhibition and Similarity*

A first list will be more poorly recalled as the strength of the tendency for items of a second list to generalize with it increases.

Suppose that a first list has been learned, and has been followed by a second list whose stimulus items potentially generalize with list 1. It is assumed that list 2 has been learned to some criterion. Now suppose that a recall of list 1 is asked for. When the stimulus items of list 1 are again presented, responses from list 2 will tend to occur by generalization (see Fig. 2, p. 199). According to postulate 2, the right excitatory tendencies will be weakened in proportion to the strength of the generalizing tendencies. Therefore, the recall of list 1 should suffer in proportion to the strength of the tendency for items of list 2 to generalize with it.[6]

The situation described is of course the one which results in

[6] There is a further possibility of explaining a decrement in recall of list 1. As list 2 is learned, items from list 1 tend to occur, and must be differentiated from the list 2 items. Suppose that this differentiation actually involves *inhibition* of the generalized responses. Then the more list 1 tends to generalize with list 2, the more items from list 1 will be inhibited. When list 1 is returned to, not only will list 2 items tend to occur by generalization, but also there will be spread of inhibition for the responses of list 1—in this case, the desired responses. Whether this possibility should be taken seriously depends partly on whether generalization of responses from list 2 is sufficient to account for the loss. A comparison of the loss in this situation with that in the situation described in proposition VIII should be relevant, for in that situation there could be no added inhibition with which to reckon. If recall of list 2 is not as poor as recall of list 1 after the same interval, then some further factor such as inhibition must be at work in the present situation.

'retroactive inhibition.' It is an accepted fact that retroactive inhibition is a function of similarity of original and interpolated tasks (see Britt, **2**, p. 389 ff.). However, degree of similarity has usually been estimated on an *a priori* basis. An experiment by the writer (**8**) has checked the prediction that a generalization gradient will correspond with a gradient of retroactive inhibition.

Degree of Learning

X. *Interference and Degree of Learning*

Difficulty of learning a second list will vary with the degree of learning of the preceding list, increasing to a peak and then decreasing as degree of learning of list 1 increases.

Suppose that two lists, the items of which tend to generalize with one another, are to be learned in immediate succession. The tendency for items in the first list to generalize will *increase* to a maximum during the early stages of practice of the list, but from this point on, the tendency to generalize will *decrease* (postulate 4). Now a second list will be harder to learn as the strength of the tendency for items of list 1 to generalize with it increases (proposition VII), so that difficulty of learning list 2 should *increase* with the practice of list 1 up to the point of maximum generalization. But as the stage of learning of list 1 passes this point, differentiation begins, and postulate 10 will apply—if differentiation has been set up in list 1, there will be less tendency for its members to generalize with list 2, the decrease in generalization being proportional to the amount of reinforcement given. Therefore, the difficulty of learning list 2 will *decrease* as differentiation in list 1 is carried further.

A theoretical curve of difficulty in learning list 2 as a function of degree of learning of list 1 is plotted in Fig. 9. The prediction assumes that learning of the second list is begun soon after cessation of practice on the first list. In the curve, the early increase in difficulty due to increasing generalization is represented by the rise from *A* to *B*, and the later decrease, due to increasing differentiation, is represented by the fall from *B* to *C*. The curve is drawn to show a sharp rise from *A* to *B* and a positively accelerated drop from *B* to *C*, since an experiment of the author's (**7**) demonstrated a curve of this sort for intra-list generalization during the progress of learning.

No check of this prediction is available, since the experiments in the field (**18, 3**) have used identical, rather than generalizing stimulus members in the two lists.

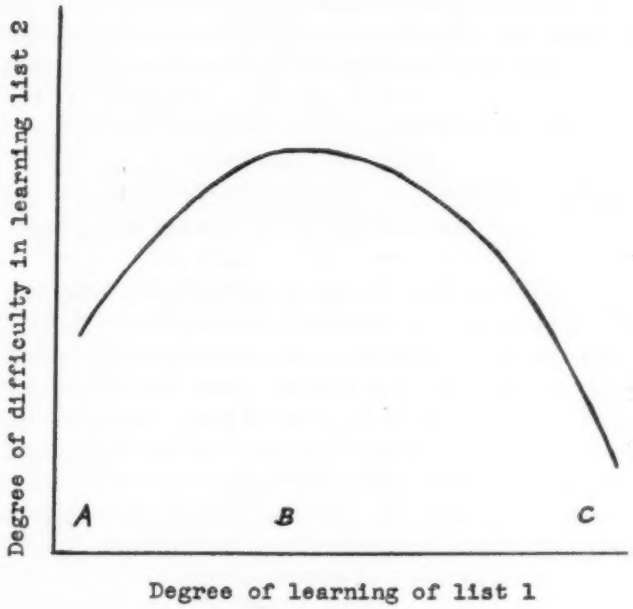

FIG. 9

XI. *Retroaction as a Function of Degree of Interpolated Learning*

Retroactive inhibition for a list 1 will increase to a maximum and then decrease as degree of learning of an interpolated list 2 increases.

Suppose that two lists, having generalizing stimulus items, are to be learned in immediate succession, and that recall of list 1 is then to be tested. If degree of learning of list 1 is kept constant, and if degree of learning of list 2 is varied, retroactive inhibition in recalling and relearning list 1 should increase with practice of list 2 up to the point of maximum generalization, and then decrease as the degree of learning of list 2 increases. The argument is the same as that given in proposition X.

The point of maximum generalization in this situation will come earlier or later depending on how well list 1 has been learned, and on time-intervals, so that an accurate theoretical curve cannot be drawn. But in any case, the inflection predicted above should occur. McGeoch (23) has confirmed the first half of this prediction; and there is some indication that the second half might have been upheld had degree of interpolated learning been carried higher, since

retroaction did not increase with degree of learning beyond the point at which the degree of learning of lists 1 and 2 was equal. Melton and McQueen-Irwin (29) definitely find the inflection.

XII. *Retroaction as a Function of Degree of Original Learning*

Retroactive inhibition for a list 1 will decrease as degree of original learning of the list is increased beyond the point of maximum generalization.

Suppose that two lists, having generalizing stimulus items, are to be learned in immediate succession and that recall of list 1 is then to be tested; degree of learning of list 2 is to be kept constant while degree of learning of list 1 is varied. Now, the initial *increase* in generalization as list 1 is given more practice will cause no corresponding increase in retroactive inhibition in this situation, because the *effective* generalization caused by this increase will be from list 1 to list 2 rather than from list 2 to list 1 (*i.e.*, wrong responses will tend increasingly to occur in list 2, but list 1 will not become more susceptible to generalization of responses from list 2, because degree of learning in that list remains constant). On the other hand, differentiation in list 1 will increase with degree of learning after the peak of generalization has been reached (postulate 4); and if differentiation has been set up among a number of stimulus items (in list 1, here), there will be less tendency for new stimulus items (from list 2, here) to generalize with them, the decrease in generalization being proportional to the amount of reinforcement given (postulate 10). Since also retroactive inhibition is proportional to the strength of the tendency for members of list 2 to generalize with members of list 1 (proposition IX), it follows that retroactive inhibition will decrease as degree of learning of list 1 is carried beyond the peak of generalization.

McGeoch (22) has shown that degree of retroactive inhibition varies inversely with degree of learning of the first list, when the lists are made up of nonsense syllables and learned by the anticipation method.

Length of Interval

Many predictions can be made with reference to the effect of varying the length of the interval between learning and recall of a list, or between a first list and a second. Only a few of the predictions will be included here as examples.[7] These predictions depend

[7] For others, see dissertation on file in Yale University Library (9).

principally on the notion of spontaneous recovery of generalization. But since the rate of spontaneous recovery will vary with the amount of differential reinforcement which has been applied, the predictions must always be stated in terms of two variables—length of the interval and amount of reinforcement or, roughly, degree of learning.

XIII. *Interference as a Function of Length of Interval Between Tasks*

The interval after which a list 2 must be introduced in order to obtain the maximum interference in learning it will vary with the degree of learning of list 1, being zero if degree of learning has been carried only as far as the peak of generalization or below, and thereafter occurring the later, the higher the degree of learning of list 1.

Suppose that two lists are to be learned, and that there is potential generalization of the items of the two lists. Now, if learning of list 1 is carried only to the point of maximum generalization, or below, maximum interference with list 2 will occur *immediately*, since there can in this case be no recovery of generalization in list 1, and since, according to proposition VII, interference will be proportional to the degree of generalization.

On the other hand, suppose that learning of list 1 is carried beyond the peak of generalization. Differentiation will increase as practice is continued beyond this point (postulate 4), and differentiation will protect the items from generalizing with a new list (postulate 10) in proportion to the amount of differential reinforcement given. But spontaneous recovery of the generalization will eventually occur (postulate 7). It will occur more slowly, the more differential reinforcement has been applied (postulate 8). So, the farther differential reinforcement has been carried, the later the maximum interference will occur.

Figure 10 shows the theoretical relationship. Maximum interference in learning list 2 will be obtained if it is introduced at once, when the first list has been learned only to the peak of generalization or below; the curve of interference would probably fall off, then, as the interval increases. But if list 1 has been learned to a high degree, little interference would occur at once, but rather would be postponed until spontaneous recovery of the generalization occurred; it might then fall off again. The rate of rise and fall represented in the curves is perfectly arbitrary, since it depends on the rate at which spontaneous recovery occurs and the rate at which generalization tendencies are forgotten, and quantitative values for these fac-

tors can only be determined experimentally. No thorough test of these predictions exists, but an experiment of Lepley's (21) indicates that, with a high degree of learning, interference will increase to a maximum, and then decrease as the interval between list 1 and list 2 increases. This result is consistent with the second half of the above prediction. Under the conditions of Lepley's experiment, the peak of interference came at about 3 hours. In a more recent experiment by Bunch and McCraven (5), no tendency was found for the degree of transfer to change with varying length of interval,

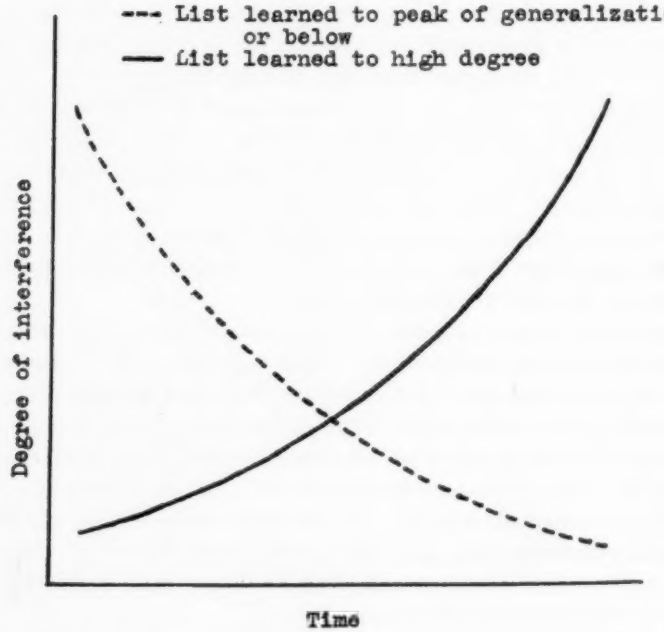

FIG. 10

although syllable lists were learned to a high degree; but no tests were made between the zero interval and 48 hours, so that there is really no conflict with Lepley's results.[8]

XIV. *Retroaction as a Function of Interval between Tasks*

The interval after which maximum retroactive inhibition will occur for a list 1 will vary with the degree of learning of list 2, being zero if

[8] In Bunch and McCraven's experiment, the total transfer effect was positive; this would not, however, prevent relative degrees of interference from showing up.

degree of learning of list 2 has been carried only as far as the peak of generalization or below, and thereafter occurring the later, the higher the degree of learning of list 2.

Suppose that two lists are to be learned in immediate succession, and that there is potential generalization between the two lists; and that retention of list 1 is to be tested after a variable interval. If the degree of learning of list 2 is low (reaching the peak of generalization or below) maximum retroactive inhibition should occur immediately, by the argument in proposition XIII above. But if the degree of learning of list 2 is high, maximum retroactive inhibition should occur later, again following the argument in proposition XIII.

A similar prediction can be made if the degree of learning of list 1 is varied, instead of list 2. No test of the temporal course of retroactive inhibition as a function of the degree of learning of the two lists has been made. But McGeoch (**24**, **25**) and McGeoch and McKinney (**26**, **27**) have investigated the course of inhibitory effects with time, and have not found a consistent tendency for retroaction to vary with time. Two of their experiments gave some indication of an increase of retroaction with lengthening of the interval, one showed no tendency for retroactive inhibition to vary uniformly in any direction, and a fourth showed a slight tendency for retroactive inhibition to decrease with time. Different materials, learning methods, and degrees of learning were employed in these experiments. The apparent inconsistency of the results points to the need for a further experiment in which temporal course of retroactive inhibition is studied in relation to degree of learning.

On the basis of this proposition, and assuming a constant interval between learning list 1 and recalling it, it is possible to make predictions for the well-known 'point of interpolation' problem, with retroactive inhibition as a function of both interval and degree of learning (**9**). Results from a number of experiments on this problem are superficially conflicting (**2**, 399 ff.; **38**). High degrees of retroactive inhibition have been obtained by one investigator or another with the second list interpolated at almost any point in the interval between learning and recall of list 1. The discrepancy may be due, among other things, to the variable degrees of learning employed in different experiments on point of interpolation. The expectation will vary, depending on whether learning is carried to a low or a high degree.

Meaningful versus Nonsense Material

This group of predictions is related to the rest of the hypothesis through the definition of *meaning* which we have accepted. Meaning has been defined as one characteristic of a verbal or visual item which serves to *differentiate* it from other items—in other words, generalization is at a minimum for meaningful material, unless it occurs on a secondary basis, as in the case of synonyms (34). Proposition XV is included here because the pattern of the argument is similar to that in the predictions actually dealing with meaningful material.

XV. *Learning of Pre-differentiated Items*

If differentiation has been set up within a list, less generalization will occur in learning a new list which includes the same stimulus items paired with different responses; and the trials required to learn the new list will tend to be reduced by reduction of the internal generalization.

Suppose that a list 1 has been learned, and is followed by a list 2 whose stimulus items are the same as those in list 1, although the responses are different. Less total intra-list generalization should occur in learning list 2 than in list 1, because if differentiation has been set up among a number of stimulus items, it will be easier to differentiate them again later, even though they are paired with different responses (postulate 9). Furthermore, since the number of trials required to learn a list increases as the degree of generalization within the list increases, the decrease of generalization in list 2 should tend to reduce the number of learning trials.

A clear case cannot be made for the second half of the above prediction, because a new factor, inter-list conflict of responses, is present in list 2 as well as reduced intra-list conflict. Figure 11

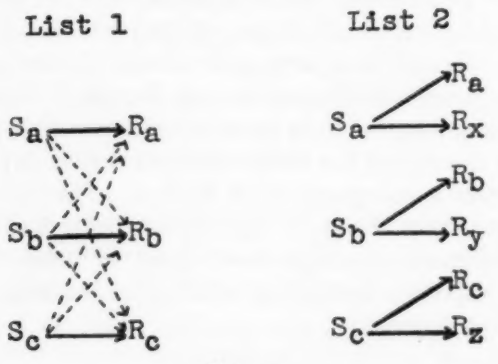

Fig. 11

shows the original generalizing tendencies in list 1; these have been successfully inhibited before list 2 is undertaken. Therefore, no generalizing tendencies are represented in list 2; but tendencies for the stimuli to excite their first-learned responses as well as the new ones are represented in list 2. A control group which learned only list 2 would have the generalizing tendencies (as represented in list 1) to contend with, but inter-list conflict would be absent. It is impossible to say whether number of learning trials would be actually reduced in the case represented, without knowing quantitative weights of the two factors. The facilitating effect of reduction of generalization within the list would be pitted against 'associative inhibition,' to use the traditional name for conflict of learned responses to the same stimulus.

The first half of the prediction can be verified by comparing the number of cases of overt generalization (confusion errors) in the first and second lists. A reduction in such errors was found in an experiment of the writer's (8).

XVI. *Ease of Learning Meaningful Lists*

More trials will be required to learn a list in which the stimulus items are nonsense syllables, than one in which they are meaningful words.[9]

Suppose that two lists are to be learned which vary only in that the stimulus items of one list are meaningful words, while those of the other list are nonsense syllables, or relatively meaningless. According to definition 8, meaning is a characteristic of a verbal or visual item which serves to differentiate it from other items. Furthermore, if stimulus items have once been differentiated, it will be easier to differentiate them again later, even though they are paired with different responses than those learned when the original differentiation was set up (postulate 9). Therefore, more generalization will tend to occur in the list whose stimulus items are less meaningful; and since as the degree of generalization within a list is increased, more trials are required to learn the list (proposition IV), more trials will be required to learn the list whose stimulus items are less meaningful.

The fact that a list of meaningful words is easier to learn than one made up of nonsense syllables is notorious, and it seems certain

[9] An exception to this prediction should be a case in which the stimulus items are *mutually synonymous* meaningful words. This situation has actually been shown, in the Smith College Laboratory, to increase learning difficulty as compared with unrelated stimulus words.

that a check of the situation described above would yield positive results.

XVII. *Interference and Meaningfulness*

More trials will be required to learn a second list when the stimulus items of both first and second list are nonsense syllables, than when they are meaningful words.

Suppose that a second list is to be learned following a first list, and that ease of learning list 2 is to be compared when stimulus items of both lists are nonsense syllables, or relatively meaningless, with the case when stimulus items of both are meaningful words. In the former case, generalization from list 1 to list 2 should be greater than in the latter, since meaning serves to differentiate the items. And since more trials will be required to learn a second list in proportion to the strength of the tendency for items of a first list to generalize with it, more learning trials will be required when the stimulus items are relatively meaningless.

In a check of this prediction, it would be necessary to prevent such factors as overlearning in the nonsense syllable situation from cutting across the main variable (relative meaningfulness) and confusing the issue. If learning were carried to a criterion, many more trials would presumably be given the first list when stimulus items were meaningless, thus introducing a second variable, and incidentally tending to even up the two situations as regards degree of differentiation reached in the first list. This difficulty might be avoided by delaying presentation of list 2 for a length of time sufficient for spontaneous recovery to occur. Also, comparisons should in each case be made with control groups which have learned only list 2, but are otherwise identical with the experimental situations described, since list 2 would presumably be harder to learn when stimuli were nonsense syllables, even in the absence of list 1. Degree of relative negative transfer would be calculated separately for the two situations, and then compared.

XVIII. *Retroaction and Meaningfulness*

Retroactive inhibition will be greater for a first list when the stimulus items of both first and second list are nonsense syllables than when they are meaningful words.

Suppose that retention of a first list is to be tested after interpolation of a second list in two cases: one, when stimulus items of both lists are nonsense syllables; and two, when stimulus items of

both are meaningful words. More retroactive inhibition would be expected in the first case, by an argument similar to the one above.

Again, a number of variables other than the crucial one will tend to cut across the experimental situation, and it would be absolutely essential in a test of the prediction to provide control groups in which interpolated learning was omitted, so that actual percentages of retroactive inhibition could be calculated for the two cases. In an experiment by Sisson (**37**), retention was compared for list 1 when both lists were made up of syllables of 100 per cent associative value with a case in which both contained only syllables of 0 per cent associative value. Presumably, a higher associative value is correlated with greater meaningfulness of the item. Sisson found slightly poorer retention in the first case; but percentages of retroactive inhibition could not be calculated, since no control groups were provided. More learning presentations were given 0 per cent lists, since a learning criterion was used. Time intervals between tasks were brief. This experiment should be repeated with adequate control groups, since it has certainly been generally assumed by investigators that the surest and highest percentages of retroaction are to be found with nonsense material.[10]

Possible Extensions of the Hypothesis

It has been possible to present only a sample of the predictions and implications of the hypothesis advanced. Many extensions may be made, especially if one or two new postulates are added. Among the potential extensions is a group of predictions involving various phenomena relating to overlearning. These predictions require a definition of overlearning, as well as the new assumption that the *variability* of trials needed to learn individual items will increase with aggregate generalization. Six or more predictions follow, among them these: increase of overlearning with greater generalization; later maximal loss as a result of high generalization; various length-of-list phenomena, such as decrease in retroaction with increasing length of list; decrease of retroaction as generalization *within* a first list increases.

If a postulate concerning 'disinhibition' be added, a num-

[10] If the material is really highly differentiated, being not only meaningful words, but, furthermore, words making up prose passages or poerty, rather small percentages of retroaction are apt to be obtained (**26**, **27**).

ber of predictions regarding the effect of shock or distraction in learning and retroaction situations will follow. A further group of predictions ensue if an assumption regarding the effects of caffeine be included. These particular extensions have been mentioned, because they have been worked out in (9).

Evaluation

A hypothesis relating certain facts of verbal learning to the more general concepts of differentiation and generalization has been presented. Following this, a number of propositions dependent on the hypothesis were outlined in some detail. Since the potential number of deductions from a hypothesis may be almost infinite, it does not constitute a perfect evaluation of that hypothesis to calculate the percentage of verified propositions which have been shown to follow from it. Yet, it is certainly of interest to examine the evidence so far obtained, since negative results, at least, are a good indication that something is wrong. Where evidence relevant to the proposition was available, it has been referred to already. To summarize, conclusive positive evidence exists for eight of the propositions presented; some positive evidence exists for four others, though not conclusive; no evidence exists for four; and for the other two, the existing evidence is conflicting and not entirely relevant. In other words, none of these propositions has been shown to be false. Furthermore, of the propositions which have been worked out but not included here, none has been shown to be false.

A hypothesis may well be evaluated with reference to its scope, also—and in this connection, the types of learning phenomena which the hypothesis is not adapted to explain, as well as those which it can handle well, should be mentioned. Obviously, the hypothesis is framed to deal with that aspect of verbal learning in which development of discrimination between items is vital; and it is limited to a stimulus-response type of analysis. Facts relating to various types of transfer-effect, both intra-list and inter-list, constitute its principal applications. Many other facts of verbal learning are not

predictable from the hypothesis as it stands. A number of these facts (serial position effects, reminiscence, etc.) can probably best be subsumed under a hypothesis which develops the implications of the *inhibitory* aspects of mechanisms postulated as basic to learning, and the present hypothesis has left unexplored such possibilities.[11] Other facts of verbal learning not subsumed by the present hypothesis would be facts of recognition-memory, and memory 'changes.' Here a perceptual analysis, perhaps of the type made by the Gestalt psychologists (**19**, **20**, **32**) might be more profitable than a stimulus-response analysis. It may be that the problems of verbal learning must be approached from both these angles, at the present time at least. In spite of these limitations, the writer feels that the range of the present hypothesis is satisfactory, since no more comprehensive theory centered around those problems of learning which are broadly classifiable as 'transfer' phenomena seems to have been proposed. The value of emphasizing discrimination as a factor in verbal learning seems unquestionable; and that generalization and differentiation are concepts suitable for its systematic development seems almost as likely.

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[11] The 'Lepley hypothesis' (**21**, **14**) covers some of these facts by exploitation of a postulated 'inhibition of delay.'

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OBSERVABLE BEHAVIORS

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In a recent paper in the PSYCHOLOGICAL REVIEW (**23**) in which he discussed types of constructional gackground for psychology Professor Reiser cited with significant emphasis Eddington's remark that the electron by itself has no physical properties and that there would be nothing whatever to say about it if it were absolutely alone, not even that it was an electron. The electron does not renew on its smaller scale the status of the old atom-thing, but enters, we may say, as an ingredient of a new type, modifying the atom even to the extent of extirpating its ancient boundaries and manners of localization in Newtonian space. An electron not only requires a universe for its adequate description, but it proceeds to dissolve the very walls of its former homestead.

Now we know very well that psychology has had to struggle all through its scientific history with certain boundary-transcending characteristics of the phenomena it has under investigation; also that the spatial localizations it has assumed for these phenomena in the Newtonian scheme of things have remained dubious wherever anything more than vague generality of reference has been required. Man's seeings, desirings, knowings, purposings, and plannings leap towards objectives in defiance of Newtonian boundaries in space and time, and man acts thus 'at a distance' in the sense that 'connective'[1] processes of Newtonian types cannot be precisely traced in research, however they may be indicated or believed to be collateral. This region of psychology's

[1] The word 'connection' will be employed throughout this paper in the technical sense proposed by Dewey in his *Logic* (**12**, p. 55). It is there brought into system with the words 'reference' and 'relation.' We may understand as follows:

Connection: any thing-to-thing event, whether its report is the outcome of physical, physiological, or behavioral research.

Reference: any word-to-thing event, such as naming.

Relation: any word-to-word event, such as symbolizing.

difficulty is labelled that of subject-object (*S-O*)[2]; it once aroused endless discussion in terms of mind *vs.* matter, though the issue in this form retains little interest and no value at all for us today.

Take perception as an example, and take as a test our ability to make a firm report upon significant fact. We possess large accumulations of approximately envisaged fact, and it is easy enough for our blunt approaches, whether by way of psychical or physical terminologies, to erupt in doctrinaire assertion. But cautious and discriminating inquiry running direct to the point has never yet been able to maintain clear distinction between perceived object, mental percept, and perceiving process, although all three of these possibilities of factuality force themselves upon attention, along with their conflicting implications of 'outer,' 'inner,' and 'connector.' This vagueness is *fully as great today* as at any time in the past. The confusion is still more troublesome for other psychological phenomena such as those labelled concepts, thoughts, ideas, meanings and knowledges.[3] It is present no matter what form of 'mental' or 'sub-mental' is used, and equally when the whole flock of 'mentals' is expelled and some form of 'mental substitute' inserted. The phrasings of the dictionaries, text-books, and laboratory reports all offer overwhelming evidence.

The great task of psychologists, as of all other scientists, is to observe. Precision of observation is their goal. Without it their science stumbles. If physicists in the development of their observation have found themselves transcending the atomic pattern, and if psychologists in their attempts to

[2] Understand the use of the abbreviation '*S-O*' in system with two companion abbreviations as follows:

S-O: the subject-object differentiation in its more general expressions: popular, tentatively psychological, or philosophical.

P-E: any current form of differentiation between person and environment, useful as broad description, but still lacking close specification for inquiry.

Org-obj: the recent naturalistic confrontations of organism and object in behavioral research.

[3] For the status of 'belief' and of 'desire' note what steps Dewey has been forced to take in the development of logic and of the theory of value (**12**, p. 7; **13**, p. 16).

copy atomic patterns have never attained to an adequate, specific observation of a definite behavior, located in a definitely determinable time and place, nor to one which (except in make-believe) can be precisely attached to a definite name valid throughout research, then it may well be that the physicists' most recent experiences with atom and electron have, methodologically, an import for us today.

I wish briefly to examine some of the problems and difficulties that are involved, and some of the possible outcomes. We have to do with the types of observation psychologists have made, or have endeavored to make, or have believed they were making in the past, with the deficiencies they have revealed in these types of observation, and with certain newer types they are now attempting to make or believe they find indicated for the near future.

Observable behaviors will be our subject of inquiry. Preliminary to such inquiry we must have a tentative, introductory accord as to what we are to understand by 'observation': an inspection of what Bridgman calls 'text'—the context of our assertions—the texture of our inquiry. The word 'observation' is itself a psychological term, naming psychological fact; as such it is subject to the characteristic psychological indefiniteness above indicated with respect to its own proper factual-observational status. We have many imperfect theories of observation which, when we take them too seriously, interfere with us more than they aid. If we refrain from emphasizing such theories too strongly, and if we postpone quibbles over the imperfections of our immediate statement and consider for the moment solely our need of *preliminary* orientation, then perhaps those of us who occupy central ground as to scientific method may agree on certain provisional attitudes. Let it be understood that our resulting conclusions will be qualified as subject to the validity of the method of observation here adopted, though this latter in turn is to be checked back from the conclusions under the ordinary tests of coherence.

Our provisional attitudes towards observation will be as follows:

I. *Data upon Observation*

Scientific observation is concerned, not with generalities, but in each case with *an* item, *a* thing, a *specific* fact or candidate for factual status.

It has direction.

Its direction is engineered or maintained—to use a roughly descriptive expression—through signs, where by 'signs' we understand behavioral connections with a range all the way from sense-stimulation to symbol.

These signs have cultural-historical connection with previous observations—our own, and those of others.

In highly developed knowledge the linguistic signs, and among these peculiarly the mathematical, have outstanding importance.

All these signs, sensational, perceptional, verbal, symbolic, show ordering or pattern (again to employ roughly approximate expression); our observations are thus patterned; the closer knit they are, the more dependable the science.

Since they arise in naturalistic evolution which we take to be an unfinished process, observations cannot safely or sanely claim perfection and finality among their characteristics. Observation grows.

We may further say that under the above approach no *fundamental* cleavage of one realm of observation from another rises, nor of observation from interpretation, nor of description from theory. Child under mother's guidance looking with bare eyes at monkey in zoo observes within the scheme of an ancient naturalistic system of physiological organization and cultural growth. Microbiologist in laboratory and astronomer reading photographic plate observe in a naturalistic system, naturalistically expanded from what came before. The many differences of detail remain greatly in need of investigation, but distinctions stressed as 'fundamental' are sentimentalities to be dropped—or at least they will be so regarded here—since in neither early nor late observation, whether simple or profound, may we assume (scientifically speaking) that we possess exclusive or even predominant hold on what folks in folkish confidence call 'reality.'

If the above characterization may seem to lack certain of the everyday stresses on observation, it emphasizes in their place the important and necessary features both of everyday and of scientific procedure. Scientific observation strains above all at definiteness: *the* fact which it observes must be observable at *a* place and at *a* time. This approach is basically quantitative, even though employing, as it sometimes may, no other number than 'one.'[4]

An electron is definitely observable under this attitude and approach. The fact that nothing can be said about it

[4] Unfortunately much laboratory research and statistical technique is at times wasted on material that contains no observable datum capable of specification in terms of 'a' or 'one.' A recently fashionable series of investigations into '*the* concept' is very much in point. What precisely is meant by '*a* concept' one can nowhere discover (14, 15, 16, 17, 18, 19, 20, 24, 25, 26).

solus—I shall occasionally use this word in reminiscence of Eddington's 'by itself' and 'absolutely alone'—does not obliterate the vastly more important fact that within its own proper system of observation so much of the utmost scientific interest and importance actually *is* being said. *An* electron cannot be discarded as a mere 'construct,' for to do so is to attempt to judge and sentence it in terms of a defective 'mentalistic' theory of observation. An electron, to be observable, requires its own proper factual background which happens to be one, not of Newtonian space but of relativity, and not of Newtonian mechanics but of undulations. Thus observed, an electron is given its measurement and localization. Thus established, experiment and observation reenforce each other to greater exactness for its further examination. Herein lies its factual and observational definiteness.

How will *a* behavior stand as *an* item of knowledge when comparably considered? This is not a question of analogy, but of method, and especially of the method of free research. If psychology is to advance as a science, and if scientific advance rests in observation (which in the older way of talking is merely to say: if *sensus* and *intellectus* go ahead together), then certainly psychological observation will in time come to concentrate upon definite behavioral facts and things which it can definitely show as belonging to its own subject-matters. The old generalities of expression will no longer suffice. For this purpose psychologists will require and must be allowed their own observational base, and this base from time immemorial, psychologically speaking, has sought to take into account the boundary-transcending characteristics of psychological subject-matters. Presumption, then, should strongly favor the search for special instances of *a* behavior, as *observable*, having *localization* in psychology's own proper background of knowledge. However suspicious this may look from certain earlier viewpoints, it amounts simply to saying that if the word 'mental' has any factual reference at all, our business, scientifically, is to observe and study this factuality.

I shall employ henceforth the expression Observation Base [5] to name the texture of thing-observed in its observational setting, without either distorting the background of observation into 'construct' or the observation itself into 'inference.'

We may begin by tabulating certain of the more striking stages in the advance of the older sciences as usable knowledge, pretending neither to expertness, completeness, or precision, nor even to accuracy in dating the stages, but holding steadily in view the Observation Bases employed and their transformations.

In the case of physics we find:

II. *Data upon Physical Observables*

A pre-scientific view of thing-acting—as spirit, form, or force—benevolently, malevolently, or even neutrally; with Molière's *vis dormativa* a fair example in a late charlatan survival.

A powerful, sustained observation of gross motions—Galileo's is the name we stress.

Newton's third law: equation of action and reaction in an organization [6] of contraposed mass-things taken as present in a rigid, independently 'existent' space and time; the old 'force' now becoming reduced to the status of factual description.

Energy entering, becoming seemingly the dominant 'real,' then being transformed into a characteristic of physical system.[7]

Light, once seen as instantaneous and then as simple velocity, coming to establish itself, along with other undulation, as fixed maximum velocity, and as such no longer a proper member of the family of speeds bred out of space by time.

Newtonian space and time ceasing thereupon to be rigid separables and (as approximations) becoming constituents of system.

Wave formulations joining, and for the present at least showing signs of dominating, particle formulations.

Radiation, once an alien among the 'things' of physics, becoming thus its now dominantly factual 'thing.'

In the sciences of life comparable changes show themselves, though here rather as overlapping attitudes of approach, the trend of which, however, is plainly visible:

[5] Since Observation Bases rise and decline as stages of science, not as realities or unrealities, truths or falsities, we may use the qualification *Nascent* for any old Observation Base as it may be viewed from our present standpoint, or for any present Base as we may appraise it with an eye to the future possibilities of knowledge. Thus, as we shall later note, an Observation Base as 'force' has been historically nascent to a Base as 'thing-*solus*,' and this latter to one as 'system.'

[6] Understand this word to indicate any scientific development in terms of things-*solus*.

[7] *System:* Understand this word to indicate any scientific development methodologically comparable to the treatment of the electron by physics.

III. *Data upon Organic Observables*

The animal organism, together with the human, is included in natural evolution.

Vitalisms, once strongly favored, pass under steady replacement.

Physiological organization presents itself as competent to do the needed work.

Ecologies prepare the way for widened organization running beyond the more narrowly physiological.

Primary data begin to be observed in durational forms, complementing and threatening possible replacement of the static-structural.

Atom-patterned lines of causation begin to yield to system-patterned, as in Coghill's work with Amblystoma (11).

The gene ceases to breathe fire through its nostrils in pretentious independence, and begins to become reconciled to a place within the system of life.

Pavlov makes specific extension of physiology beyond the human skin so as to include 'outer' facts directly within the organism's working process, forecasting thus a wide extension of inquiry (3, 311ff).

Still without stress upon historical order, but concentrating upon types of Observation Base employed, and avoiding control by ordinary language-conventions as well as by any insistent dogma, we may note among the phases of the advance of psychology as science the following:

IV. *Data upon Psychological Observables*

An initial period of Souls and Spirits, enjoying power, initiative, and responsibility.

The failure of behavioral phenomena (the lookings before and after, the purposings, communicatings, influencings, and idealizings) to exhibit causal connections of types determinable for physical phenomena in Newtonian space and time; their distinctive display of 'boundary-transcending' and 'distant-acting' characteristics, and their consequent status (in the scientist's preliminary inspection, at least) as a group of remainders left over from the world of Galilean and Newtonian motions.

Assignment of these remainders to an Observation Base taken in divorce from the physical, and lumping together the contrasts of spirit to body, of subjective to objective, and of psychical to physical.

A long period in which 'mind-body' was the dominant formulation for the boundary-transcending behavioral problems.

A comparatively early determination by psychologists that when they used 'Mind' as their Observation Base, no item such as *a* mind could be observed or cleanly examined by them as *a* fact; this being followed by the abandonment as non-observable (though with the common retention of many accompanying language-forms for purposes of rough description) of all hypothetized 'faculties' of mind, down even to the late revival of a facultative 'I' in an I.Q.

'Mental' or 'psychical' *particles* viewed on the atomic model in their turn fail to attain definite experimental observation as specific items. Note: the chaotic flutterings of '*an* idea,' notorious ever since Locke; the recent rise and fall of '*an* instinct' (regarded as a 'mental' item, rather than as a rough verbal description); the 'emotion,' since James and Lange insisting upon physiological observation; the tech-

nical Titchener 'sensation,' not able to maintain a usable status in inquiry; 'feeling' alone retaining fairly sound determinability, but this rather as physiological than as behavioral description, even where the 'psychical' label remains attached to it.

The 'mind-body' formulation now loses interest, and the direct use of a 'mental' or 'psychical' Observation Base comes to be abandoned; this occurring precisely because the 'mental' has failed to yield dependable observations of definite facts.

The boundary-transcending subject-object (*S-O*) phenomena remain as much as ever in need of an adequate Observation Base. Their 'connections' are not physical in the technical scientific sense of that term, and since they are no longer referable to 'powers' of the 'mind' or to the magic of mind-bits, they cry even more loudly than before for a direct inquiry.

As a tentative approach, however, prior to the direct inquiry, a substitute for the old 'mental' is sought in the overt mechanistic movements which the original Behaviorists took as their Observation Base. Good as far as it went this substitute did not reach far enough to have lasting importance.

Another substitute for the 'mental' as Observation Base is then tried, namely, physiology. The physiology itself is fine, but it can only report on *S-O* behaviors by assuming them as collateral, helplessly and harmlessly, to nerves and brains. Its reports never reach behavioral phenomena proper, and the Observation Base it establishes remains physiological instead of becoming psychological.

Various 'whole-organisms,' 'sub-mental' Freudian items, emergent shadow-mentals, and ideologies capitalized as 'causes' are found, all of them makeshifts for observation in clean-cut naming.

Enormous laboratory accumulations of nascent observations mostly proclaimed with thing-*solus* assertiveness, are at hand awaiting directly coherent behavioral formulation and perfected organization upon an adequate Observation Base of their own.

To sum up for all three realms of scientific inquiry:

V. *Summary for Changing Observation Bases*

Physics has transformed its Observation Base from one of initiating powers or forces, through one of things-*solus* in organization, to one of full systemic description.[8]

Biological sciences show comparable trends of transformation, though not in such sharply marked succession of stages.

Psychology, much more complexly, travels the same route. It has used 'psychical,' 'physical,' and 'physiological' Observation Bases, and in each of these (though in varying degrees of prominence) it has employed 'power' patterns and 'thing-*solus*' patterns, but has not as yet firmly established itself in any stage beyond.

Fluidly phrased, our report thus far is that psychologists do not as yet make direct, literal observation of their own primary phenomena of inquiry in the sense in which other sciences observe: not as psychical, whether as selves or as self-dust; not as physical, whether as mechanistic or as post-

[8] Or, one may say: from the arbitrary through the compulsory to system; or again: from ignorant imputation, through objectification, to factual knowledge.

mechanistic; nor yet as physiological, despite all the riches physiology has brought. Their raw material of inquiry—the behavioral itself—still fails to obtain distinctive observation of its own. The many interesting and valuable observations they make, whether in psychical, physical, or physiological form, prove on test not to be distinctively and observationally behavioral. Their science is thus ripe for an important step forward.

We shall next proceed to consider some of the newer psychological procedures that are endeavoring to overcome the defect; but since the nascent physiological base for psychology is now at the height of its popularity as confident claimant to the psychological throne, and since it is this base that provides the starting point for the next advance, we shall do well first to secure a sharper and more complete statement of the actual situation of knowledge in this region at this time:

VI. *Detail for Physiological-Psychological Observation*

1. Behavioral clues have opened wide physiological vistas.

2. The resulting physiology is fine *physiology*.

3. This physiology undoubtedly gives the death-blow (if one is still needed) to the old 'mental.'

4. This physiology has nevertheless not itself yielded (Pavlov alone excepted—and he only through experimental disregard of the normal physiological boundary-line) one single direct report or description of any characteristically behavioral *S-O* phenomenon.

5. The most competently explored neural train in the science has not yet shown a capacity in its own right to report upon any such phenomenon as 'horse-seen,' much less 'word-understood.'

6. The only way this physiology succeeds in labelling itself 'psychological' is by treating behavior as collateral to neural process.

7. How behavior is thus 'collateral' is a pressing *scientific* issue—assuming that 'science' is really a passage from guessings to knowings—and here physiology has nothing to tell us.

8. The common procedure is to employ a collateral quiddity or dummy of the old 'psychic' type, thus admitting at the back door in an inferior capacity the very nuisance that had been lustily kicked out the front.

9. Characteristic of such a quiddity is that 'behavior' is treated as some sort of an act which the dummy 'performs' at a spot—anciently a psychical spot, now a physiological spot.

10. This rates more as a change of name than as progress in primary behavioral observation and report.

11. The one requirement, namely, matching the physiology against the fully characteristic systemic behavioral process, is just what the physiology in its own

terminology does not accomplish. The 'quiddity' it offers remains as mystic as was ever the old straight-psychic, and the Observation Base it offers remains physiological.

12. The psychologist is unquestionably to be controlled by physiology in everything it establishes within its own Observation Base and by its own exact methods. Such control, we may fully believe, will endure and increase. But beyond this, for the present at least, the psychologist must seek for himself.

13. To sum up: Physiology as it now stands offers *no guarantee whatever* that the locus of *a* behavior is within a skin, nor even a first hint that that is where *a* behavior should actually and factually be sought. If or when a physiologist or any one else talks of phenomena without locus, we have no need to listen.

The newer forms of observation which we are now to examine have a right to regard themselves as attempting to deal with the psychological proper as distinct from guesses, substitutes, and approximations of language. Recently in another place (**3**) I have appraised some of these procedures. Without repeating the discussion, we may here recapitulate by displaying three typical forms of attack. It will be manifest that our cues come from Lewin, Pavlov, and Kantor, but our sketches of three cases to be known as *A*, *B*, and *C* do not hold minutely to these men, since it is types of approach to the problem rather than professional biographies that concern us. It is essential to recognize, however, that Kantor's initiative and leadership in this field is unquestioned (**21**). In comparison with him Lewin offers us a belated 'mentalist' shadow, while the peculiar significance of Pavlov's procedure could not readily have been appraised free from its ordinary mechanistic discolorations until after Kantor had established the background of observation.

Case *A*. Suppose that, under various current influences, a psychologist excellent in special research, but still wearing the linguistic harness of the old 'psychic,' should say: "Come, let us make us a 'system' in which we can hold *S-O* phenomena in union for study"; and suppose he should look around and discover a mathematical technique flexible enough for his use. Suppose he should select as components of his system, and as representatives of *S* and *O*, the two presentations 'person' and 'environment,' symbolizing them as '*P*' and '*E*', and should call their system a 'field.' Surely we will have sympathetic interest in his undertaking. But suppose our ex-

amination of his accomplishment with a view to its adoption shows it making use of three or more warring items under the symbol '*P*,' and does not enable us to become clear just what is the status of *E* with respect to *P* within the 'field' in terms of the typically physical, physiological, and behavioral issues that arise. Then we will doubtless conclude that the time has come for us, if our understanding is to progress, to concentrate upon direct inquiry into the status of the phenomena *P* and *E* themselves within the field. In a case like this, if we find that the *P* and *E* enter *solus*, then we have 'organization' rather than 'system' to investigate, and our business will be to build up the 'connections'[9] on that basis. If, however, the field is properly 'system,' as in the mathematical-physical procedures from which its name is taken, then our business is its free observation and delicate analysis into its constituent *P* and *E* or whatsoever. Here again 'connections' will be the exhibit in the outcome. Despite difficulties such as these the mere appearance upon the scene of procedures such as Case *A* must be taken as having high symptomatic import at the present stage of psychological advance.

Case *B*. We suppose a physiologist of outstanding ability who arrives in due course at experimentation with the buccal cavity on a basis comparable with his earlier work with stomach and brain. He finds himself observing animal activities involving eye, tongue, touch, smell, hearing, and other stimulus-reaction situations as direct contacts of organism and object, on a basis which we may style org-obj,[10] without drawing any *radical* distinction *in his experimental procedure* between inside-skin and outside-skin phenomena. The resulting opportunities for research prove magnificent; they justify life-long activity for himself and for scores of keen associates; even after forty years their attractions are increasing rather than declining. Our physiologist discovers and insists that whenever he makes his reports in the old 'psychic' terminology he blurs the facts, but when he adheres to straight behavioral description as he understands it, he makes a gain in precision

[9] For 'connections' refer to footnote No. 1.

[10] See footnote No. 2 for status of org-obj with respect to *P-E* and *S-O*.

of inquiry. He asserts vigorously that he is a physiologist, not any kind of a psychologist he knows of; also that as a physiologist he is expanding physiology across new territories. What concerns us here is not whether the investigator of Case *B* 'believes' or 'disbelieves' in a psychic, nor what *obiter dicta* may come from him about world and man. Rather it is the fact that when he discards the older terminology he is in effect replacing his old Observation Base with a new one; further that his new base is just what he calls it, namely, an expanded physiology (*i.e.*, expanded beyond skin-limitations), and not a physiological sentimentalization purporting to *be* in its own right a certain 'something' which it had previously discarded as *not*-being. Clearly such an exhibit is neither itself old-psychical, nor old-physical, nor old-physiological. Its range of experiment is limited, indeed, to animal perception-behaviors, and we can not be too certain as to whether its trend in the long run will be towards 'organization' or towards 'system,' but we certainly can see in it at least a possibility of full system-development in the present sense.

Case *C*. We assume a psychologist concentrating naturalistically on the materials of his inquiry who comes to the conclusion that the 'psychological fact' is to be found only where organism and environmental object confront. He finds such confrontation almost everywhere recognized as a necessary 'condition,' but this in the main grudgingly and as a handout to philosophy in the *S-O* form, and nowhere used as a building block for scientific psychological research. He determines so to use it, and he puts it definitely in the form org-obj. He proceeds thereupon to assert that for behavioral inquiry organism and object are both necessary, that they are all that is necessary, and that behavior is their 'interaction,' with the 'function' of the object—the part it plays—just as essential to any behavior as is the part played by the organism. Our psychologist we may regard as having been fortified in his purpose by his clear view of the manifest defects of current constructions: the 'psychical' can not be seen; what one sees as 'physical' is not itself in that form behavioral; the physiological may ape the behavioral, but

does not itself arrive at it. In his development he employs as locus of all his phenomena a specifically successional geologic time, and a specifically terrestrial extension of space. In this background he sees the course of natural evolution as yielding successively objects, then organisms, and still later phenomena we call behaviors—those in which objects and organisms enter into organization with one another in super-physiological style. A procedure such as that of Case *C*, appraised in the perspective afforded by our general survey of scientific Observation Bases, offers a striking parallel to the case of physics when Newton introduced his third law of motion. Things-*solus* are set up in fixed temporal and spatial frameworks, and events are presented as equated in interaction. This treatment expels violently the older 'force' regarded as that which 'puts things over,' and it replaces all such forces by descriptions, but it offers these descriptions in terms of 'things' which are assumed to be capable of independent preliminary characterization on their own accounts (in Newton's case the mass-particles, in Case *C* the existent organisms and objects) *before* the description of their interaction is undertaken.

Given Cases *A*, *B*, and *C*, we may make the following comments: In the Newtonian organization of physics there was a defect so subtle that it required two hundred years of accumulating observations before the physicists got their first hints that it was becoming necessary to transport 'masses' (and 'space' and 'time' as well) from the status of independent exterior facts or conditions into the physical 'system' proper. For psychology, in contrast, such system (*S-O*) has been all too disturbingly in evidence from the start, and psychologists have evaded it by turning it over to magic (psyche) or to the pundits (philosophy) to be dealt with, while they themselves have endeavored as best they could to imitate the still imperfect physics of their period. Only now at last is the characteristically systemic behavioral fact beginning to obtain direct investigation in the scientific form by its own right. We have Cases *A*, *B*, and *C* as types of change. But procedures of type *A* must certainly free themselves from em-

pyrean pretensions, and must operate on earth where immediate observation can be made; those of type *B*, however epoch-making, offer us as yet only a firm start; while those of type *C*, assuming any such to be found in the form we have given them, must show that their 'organization' is technically adequate to deal with *S-O* system in its more difficult ranges, if they are to become the dominant pattern of inquiry.

In our earlier discussion of developments such as the above, the word 'situational' was employed (**3**, p. 311) to name all treatments of behavior in which "the locus of the behaviors no longer is placed in the organism alone." The word was there brought into contrast with 'psychological' because of the latter word's etymological involvement with a 'psyche' now become decrepit and confined by feeble wing-power to an intra-dermal home. This contrast was solely for the convenience of the moment. 'Psychological' is, of course, the permanent name to give to all behavioral research and knowledge, of whatever type. Let us here, therefore, while still retaining the specification for 'situational' cited above, regard its application as limited to those varieties of psychological inquiry which employ a widened Observation Base in contrast with the older varieties which retain psychical, physical, or physiological localizations in or directly at a skin-bounded organism. 'Situational,' thus used, will cover cases both of organization and of system within psychology, since both employ loci wider than the organic body—the former compounding behaviors from independently specified components, the latter proposing to establish the scientifically valid differentiation of organic and objective constituents within and by means of the behavioral research-in-progress.[11]

[11] No apology is needed for altering the earlier application of 'situational.' Verbal rigidity is always our danger, and free experimentation with words our need, so long as factual report remains imperfect. 'Interaction' would be a preferable word especially because of its priority of use; but if I am right in my appraisal it is barred in the present discussion because it offers itself so emphatically in the form of 'organization' and with subordination of 'system.' 'Field' has great advantages if we could take it pure from its mathematical-physical uses, rather than as a corrupt emanation from an *S* or a *P*, devoid of all observational standing of its own either with respect to its factual or its systemic characteristics, but it is doubtful whether its originators would

Let us now establish as vividly as possible what it means for a behavior, *literally*, to be localized not in a mind or in an organism, but in a place (situation, field, 'cell,' or whatever it may be called) within which an organism holds status as at most a nucleus. Such localization may look queer at first, but this need not trouble us, since queerness is common enough in the appearance of knowledge rising wet from the pit. Actually the procedure is matter-of-fact, and can quickly be identified as such. Start with a behavior, fully, richly, concretely before you: say, John-loving-Jane, or Bolshevik-hating-Banker. Then *in your capacity as scientist, and for scientific purposes*, instead of taking it for granted in simple, naive, unquestioning innocence that you have primarily before you the separate items, John, Jane, Bolshevik, Banker, and consequently also 'the' or 'a' hate, and 'the' or 'a' love, which it becomes now your duty to patch together with sealing-wax, interpret, explain, or otherwise control by your own mental power as prime mover (or is it as fifth wheel?—you may not be at all sure), you determine to examine the situation as it comes just-so. You do some violence, indeed, to conventional attitudes and speech-forms, but it happens that under the conditions given above and for your specified purpose, namely the increase of knowledge of the type we call science, your primary goal is *not* conventionality. You quickly find as you proceed that you are gaining immensely in directness of observation, and this is what you *do* want. You will also find upon due consideration that your procedure now agrees quite remarkably with your ordinary everyday 'knowledge-of-acquaintance' of the behaviors taking place around you, and that nothing has been done away with except a conventional magic or semi-magic pretense of 'knowledge-about.' Having this much start, your job is to 'stay put' in observation, and not backslide into slovenly makeshifts as you proceed. Here now will be your difficulty. No longer may

permit us any such liberty. J. F. Brown has adopted the name 'vector-field,' and has made important forward steps in its use, especially through stimulating investigations of vision (8, 9; see also Orbison, 22). My attention also is called to the early stress by M. Bentley on the function of the environment in psychology, and to researches made under his supervision in this intent (6, 7, 10).

one fall back when puzzled upon a non-observable psyche or psyche-substitute, nor may he let the Eternal-Bodily serve as his sentimental reliance, nor require God the Great Geometer to be a purely Euclidean God. One must face the issue squarely: Are 'hate,' 'capacity to hate,' and 'hater,' 'love,' 'capacity to love,' and 'lover,' the phenomenally vivid, concrete, and 'real' scientific facts? Or should this status be given instead to the full observable hate-situations and love-situations as we can actually anywhere and anytime run across them? If this last is the case—and if we face it squarely—then 'situational' psychology presents itself at once as proper hypothesis and procedure of research.

What now as to the respective status of organization and of system within situational psychology?[12] For science generally our exhibits indicate the former as a stage in the advance towards the latter, but this is not decisive. *Solvitur operando*, one is perhaps permitted to say. The issue will not here be discussed. The difficulties psychology has always had in applying the organization method to its materials will be taken for granted as well enough known. We shall confine ourselves to a brief sketch of the possibilities of the contrasted 'situational-system' development in three regions, perception, culture, and language-thought, corresponding roughly to regions which in older days were known as senses, will, and intellect.

Take perception first. In Case *B* we considered perceptions as if directly localized in areas of a rod or so radius around a cortex, without introduction of an anatomical skin as a dominant boundary; in Case *C* we considered them, where treated, as themselves directly interactions. As background for such presentations we have much pertinent experience to draw upon. In all ages 'philosophers' have found themselves—and with few exceptions—compelled to have dealings with *S* and *O* in fusion; the bare fact of their being thus forced is informative. In science, if physics is a sound guide, the day of the old atomisms is past except for use as approximate expression in special regions. Probably every psychologist

[12] For 'organization' and 'system' refer to footnotes No. 6 and 7.

has sometime found himself recording immediacy of perceptional event, no matter how surprised at himself he feels. The word 'percept' forever winks back and forth, refusing to stay fixed, whether as *S*, or *O*, or connecting process. We forever hypothetize 'things,' which forever we must struggle to dismember again; and in behavioral regions our Occidental sentence-structure offers so many rigidities that often our hardest struggle is to set ourselves free. This last item—it is the well-known rhythm in the advance of knowledge—gives us our general justification for our course, added to the many direct clues we have secured, when we undertake an experimental technique of scientific envisionment which grasps the *S* and *O* aspects of perception together as they come, and when we propose this as a basis for still further inquiry. Elsewhere I have used Woodbridge's phrase 'sight-seen' as a verbal aid (not, however, as a valuation) in dealing with this manner of observation, affirming that it conforms to my own long-maintained personal habit whenever I take sufficient pains to strip away certain well-known linguistic distortions (**2**). Such observation, always taking it in the specific case such as tree-sight-seen, is directly 'system' in behavior. Many approximations to illustration can be found. We do not look upon tropism as involving differentiation of *S* and *O*, but still we treat it as lying in a region of transition towards such a differentiation. We do not suppose a toad to remark "Lo, me; Lo, it" before it swallows a gnat, but no more do we reduce its behavior to a presumed mechanistic pattern brought over from an older physics. Similarly (except for verbal bondage) we are under no compulsion (above all not in science) to regard tree-solus and man-solus as basic specimens of the way "God made the world," nor to place them eternally in pseudo-mechanistic contraposition to one another before we venture to ask ourselves how in fact perceptional-behavior-in-action is to be viewed. We may readily claim that we make a sound improvement in our practice when we treat the differentiation of tree and man as itself an evolutionary outcome, and our present formulation of any such differentiation as itself conditioned by the present stage and

status of evolution, including our own place in it, perceptional, cultural, linguistic, and all.[13] Even though the ordinary chores of psychology may not need any reference to this direct systemic envisagement, we are justified in employing it whenever it helps us organize those ordinary chores with one another or with other ranges of inquiry.

Turning to cultures we find that our various social sciences from anthropology to economics have already made long advances on the route towards systemic statement. We no longer ask: "How *can* a man be a Persian?" nor even: "What *causes* are needed to produce a Persian?" We are, scientifically speaking, as well satisfied with Persians as with—let us say—Americans: sometimes, perhaps, even better as more remote, and so in better perspective. One no longer demands a principle of self-interest or of greed to give a foundation to economic institutions, nor of piety for the founding of religion. One can see a savage as so-acting, and himself likewise as so-acting. A totem-pole is no longer required to be a mechanistic (or perhaps a mystic) application of a man to a tree, but can be taken directly as behavioral fact. The fuller and richer one makes his descriptions, the more he finds description itself superior to imputed exterior factors of causation. Just as has happened with physical description in respect to objects-*solus*, so in the case of behaviors: the less one knows to say about the facts, the more is he inclined to introduce an independent mental-*solus* to 'explain' them; the more one

[13] It is common to sentimentalize over our speck-like status in space and time, often, indeed, with a bit of flattery to our intellects for their modesty in thus admitting our corporeal limitations. It may be useful to consider for an instant the deeper-lying *thermal* limitations of our knowledge itself within the frame of its own report on nature. We allow our universe, physically speaking, a temperature range of many millions of degrees. However, quite unbroken accumulations of experience seem to assure us that our process of observation and report is confined to occupants of a region which has an intra-dermal thermal tolerance with a range of possibly 20 degrees Fahrenheit, an extra-dermal range of less than 100 degrees, and an extra-domiciliary range of possibly 200 degrees. How the universe might appear to beings otherwise localized with respect to temperatures is a question of no more scientific importance than whether any such otherwise localized beings 'exist.' What is important is that with respect to thermal tolerance we may appraise the validity of our ordinary dogmatic 'realisms' as something of the order of 10^{-5}; and if we are wise we may perhaps behave accordingly.

knows about the facts—in other words, the more strongly cultural research advances—the less need and the less plausibility is there for such professedly 'independent' items. Ignorance requires sops, but advancing knowledge gets along without; and in cultural research the old 'psychic' powers (and in prospect all their specious substitutes) prove to be such sops. Stylish revivals like the Freudian remain recidivist, no matter what fragments of information they assemble. The old creaky combinations of psychic with other-psychic or non-psychic items come ever more clearly to appear as cases of doubled talk, in which one says over again the same thing in changed words, without increment of meaning, under the pretense that word-reduplication is itself by some magic an explanation. All that this really represents is, however, the slovenly dodging of the painful effort of research. It thus comes about that the *S-O* separation appears rather as a subordinate tool of inquiry for cultural research than as a dependable framework of explanation. This is not to say that the majority of investigators already assert this attitude, but that the practice of the advance guard clearly shows the trend. The disappearance of the older 'forces,' which is the first step in the disappearance of all facts-*solus*, is here already far on its way, even among workers to whom the recognition of the trend, and much more its friendly acceptance, remains still abhorrent.

Finally consider the region of language-thought-knowledge. The closeness of language and thought to each other and their possible identity has been stressed since the earliest days of Greek philosophy. In era after era groups of inquirers have asserted a oneness, *almost* convincing themselves of success in demonstrating it. But opposed to these stand always other groups who strive to herd thoughts off into pure realms of wordlessness, and who in their turn *almost* believe they succeed. A dilemma like this manifestly means failure as yet to secure sufficient analysis for the situation that includes both language and thought, so that neither language nor thought can as yet attain a formulation for itself which makes sufficient allowance for the other. It is common report both

for language and for thought that each hypothetizes and that each recombines. When, however, the full complex is broken down into elements which openly or secretly are taken as basic, such as an 'I' that thinks, a 'body' that speaks, and some rigorously independent 'other-thing' that is thought of or spoken about—when we find physical-thing and concept-thing shading into one another in such way that no one can tell just where or how—when words apart from thoughts prove meaningless and thus not behavioral at all, while thoughts apart from words vanish mistily except as other words are brought in to proclaim their 'real' apartness—then the language-thought problem surely lies unsolved. Development on the basis of behavioral system offers hope, for here we shall look at the full situation directly; word, meaning, and object will begin to have their specifications developed within the inquiry. Certainly until such development is secured we can expect no success in our search for coherence, psychological, sociological, linguistic, or logical; in the last of these least of all.[14]

We have thus sight-seen, culture-fact, and language-thought-knowledge all exhibited as possibilities for study as localized in situations wider than the separate organism. As such we have them susceptible of systemic treatment in which *S-O* phenomena are approached head-on as problems, and no longer under the dominance of an isolated '*S*' which is assumed to be necessary, no matter how dubiously known. In the first of these three regions direct systemic observation is already practicable, if our inquiry needs it, and if we are willing to submit to its discipline. In the second, systemic presentation is already far along towards establishment. In the third, we have at last real hope for solving the most crucial of all our problems—that of the connection of our perceptional and cultural behaviors by way of our language-thought-

[14] John Dewey's *Logic* (12) may safely be appraised as a full generation in advance of the technical logic now currently displaying itself as half-chess and half-faith. Even his work, however, remains in large part an exploration of difficulties, with language-knowledge chaos presented as the greatest immediate obstacle to progress.

knowledge processes, themselves taken as behaviors in full system with the others.[15]

When psychology made its advance from its older psychical to its present intra-dermal physiological Observation Base, the strongest pressure in that direction came from organic evolution fresh arrived in knowledge with a sound demonstration of man's place among primate mammals. The initial inclusion of the organic body of man within nature has had this great result for improving our envisionment of behaviors.

The further advance from intra-dermal to full situational observation proceeds today under the same naturalistic pressure. The present step requires, however, much more than a curious, or grudging, or partial acceptance; it requires a full and frank adoption free from all qualifications or concealed evasions. Demanded of us now is that we recognize the absorption of all of the behaviors of men (and most emphatically for each one of us, of our very own) including all our talkings and thinkings and knowings into 'nature' in the sense of a scientifically naturalistic system of events.[16] Charles Sanders Peirce, America's greatest leader in knowledge, comprehended this the instant that he felt the impact of Darwin upon his own solid scientific equipment and Kantian protective structures, and he quickly reported it to the world in his 'pragmaticism' and 'fallibilism,' two techniques for gaining competence and stability in research which have power for the future, however they have been neglected by the past generation except in watery dilution. The verbal and other technical facilities available to Peirce in his own day for the examination of such phenomena as 'ideas' were too limited to permit him to attain his objectives, but he gave us guiding principles and took powerful steps forward which only now the world begins to follow.

A full naturalism (the 'ism' here implying merely a pro-

[15] I may perhaps add that my examination of recent psychological progress has been forced on me by my great personal need for a better understanding of language-process than I have been able to obtain by way of the older psychologies.

[16] Certain recent specimens of surviving disregard or miscomprehension are elsewhere exhibited (4, 5).

gram for observing men at work in nature) will be determinedly observational, ever modifying its reports with improving vision. The observation, however, must itself be naturalistic, and this it cannot be if it distorts its reports as if they arrived by way of 'powers,' mental, organic, or other, not themselves naturalistically under observation. Naturalistic observation in a 'nature' itself observationally established is 'system' as we have used the word. The observer professes no creed based on the assumed dominance of one single item of observation. He proceeds without any tortoise to hold up his world of research—even without dependence upon whatever testudinal mentality he has been wont to claim.

Concerning such situational system our most insistent assertion will be that it brings behavioral connection directly under inquiry.[17] Fast disappearing now will be the endeavors to compress *S-O* phenomena into an *S*-terminus, there to be studied as *S*, *P*, org, or as some alternative form, such as a mystic 'id,' a foggy 'whole-organism,' a 'cortically-potent' artery, or whatever else happens most to please the performer. Instead the *S-O* phenomena will be held steadily in view as they primarily come. Studied thus, their connection enters attention in its own right with demand for direct observational report. No longer is there need for painful struggle to explain how a 'power' of the *S* form, after it has once been set up *solus* (usually with its handful of assertively 'realistic' credentials) can possibly perform such work as it has been set up to perform. No longer will problem-solution in this field have to be turned over to experts like 'philosophers' to deal with. The work itself's the thing, the behavioral event itself, just as the event's the thing in all highly developed modern science; the *S*, *P*, or org, the *O*, *E*, or obj, must exhibit itself within it, not as above, or beneath, or otherwise presumptively fundamental to it.

In conclusion—as possible safeguard against word-distortion—the following statements are emphasized:

Observation, presented as situationally operating, has been

[17] For 'connection' refer to footnote No. 1.

frankly and openly used as implement for gaining knowledge of situational psychology. This is a 'circle,' but one without a trace of concealment. Outcomes are explicitly recognized as qualified by methods.

Observation Bases are regarded as appearing in science in successive stages, with early bases regarded as approximations on the way towards later; and likewise with future and better bases anticipated.

Whoever succeeds in establishing any piece of knowledge as resting securely in his own (or in any one else's) point-blank contact with an object will send situational psychology crashing. No general theory or creed can accomplish this but only the proffer of specific fact so established. The world as yet possesses no such fact, vehemence being still in inverse ratio to demonstration.

Situational psychology, in all of its tentative forms, asserts that its dealings with behaviors are concrete and direct, in comparison with the sundry fictions and abstractions of the older organically localized Observation Bases. It places no obstacle, and has no thought of placing obstacle, in the way of intensive research into personality and person. If one sees the organism entering as nuclear in behavior rather than as an isolated locus, one is still as free as ever to specialize to the scientific limit on nuclear studies. Such free specialization is the life of science. Sole condition is that the report of outcomes be made under qualification as to objectives and methods, and not *sub specie aeternitatis*.

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OPERATIONAL USAGE IN PSYCHOLOGY

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In an earlier article (**16**) the question of the role of the operational principle in psychology was raised. It was concluded that this technique merely restressed the need for the application of the experimental method to psychological problems. The present paper, when considered from a similar point of view, becomes a further discussion of operationism in its relation to the application of the scientific approach to the study of behavior. These additional remarks become appropriate since several proponents of operationism have not met certain of the objections raised by those who have indicated limitations in the use of the principle. The present emphasis is, accordingly, placed upon what the proponents of operationism actually *do* with this principle. Our approach, therefore, will stress neither the historical nor the philosophical antecedents of the operational principle. Rather, the paper seeks to evaluate the adequacy of research efforts as determined by current operational usage. In so doing the writers are in general agreement with all those who recognize the need for the rigorous application of the objective method in psychology. To the degree that the operational principle aids in the fulfillment of this need no question, it is felt, can or should be raised. The development of the paper will hinge upon a discussion of basic methodological criteria, subsumed in the conduction of any experiment, in relation to the operational principle. In order, however, to discuss this phase of the principle, mention should be made first of the ways in which the term "operationism" has been defined. Such an introduction may lead to a more complete understanding of the ways in which the technique has been used by representative operationists in psychology.

1. *The quest for definition.*—A survey of the recent litera-

ture indicates clearly that no general agreement has been reached by the proponents of the operational technique concerning the meaning of the fundamental principle. Representative citations will suffice to illustrate our point. Bridgman (**2**, p. 5), speaking only for the physical sciences, states in 1928 that "*the concept* [physical or mental] *is synonymous with the corresponding set of operations.*" Thus, "if we have more than one set of operations, we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations" (**2**, p. 10). In 1938, however, Bridgman (**4**, p. 116), in pointing out that the operational principle is not a theory of meaning, states that it is "dealing with a necessary as distinguished from a sufficient characterization" in giving meaning to an experimental datum. He adds (**4**, p. 129) "one can envisage here a new discipline . . ." which "will involve . . . the *selection* (italics ours) of those operations . . . recognized as fundamental." Again, he reports (p. 130) "it is an *art* (italics ours) to make the analysis emphasize the significant operations."

In another article Bridgman (**3**, p. 8) indicates the two aspects which meanings possess, namely, the "general" and the "specific" in his terminology. He states (**4**, p. 119) that "if we want to do certain kinds of things with our concepts, our concepts had better be constructed in certain ways." The selection of criteria, based on experience, makes, it seems, for specific operational meaning, since all meanings can be shown to possess degrees of general operational meaning. "Specific operational analysis does not begin to be important until we impose certain restrictions" (**4**, p. 117). These statements indicate that the *one* criterion, in the formulation of operationally meaningful concepts in the physical sciences, is what the scientist wants to do with his concepts. If this formulation of operational meanings is an art and if our purposes predetermine our operations, it is not surprising that psychologists have defined operationism in diverse ways. Stevens (**14**), for example, has said that "operationism consists simply in referring any concept for its definition to the concrete operations by which knowledge of the thing in ques-

tion is had." In line with Bridgman's (**4**, p. 129) pronouncement concerning the search for fundamental operations by trial and error procedures, Stevens maintains the act of discriminating to be even more fundamental than denoting or pointing in the giving of meaning to the concept.

With the selection of this basic criterion Pratt (**12**, p. 103) completely disagrees. Pratt holds that "the very world in which we live . . . is made utterly to disappear" by means of Stevens' statements. He goes on to state (p. 103) that "discrimination itself vanishes, for to know, and then only indirectly, that a discrimination has been made requires a further discriminatory reaction." Thus to know that which is discriminated is, according to Pratt, an impossibility since the things discriminated are, for Stevens, unknowable. But to know about the act of discrimination, Pratt (**12**, p. 105) intimates, gives one knowledge of private experience which, according to Stevens, is impenetrable. Thus, psychologists disagree on the selection of the requisite fundamental operation or operations for psychology. These quotations, bearing upon the definitions of the operational principle, support Crissman's (**6**, p. 312) recent contention that "the term operation is a coat of many colors." McGeoch (**9**, **10**), on the other hand, has apparently hailed operationism as a method by means of which agreement in psychology may be attained. Some might disagree with the thesis, however, that complete unanimity in psychology is desirable. At any rate, it would appear an unwarranted assumption that operationism, as conceived so differently by its various proponents, is the *one* method by means of which this complete agreement could, if desired, be attained. This conclusion is supported by a survey of the recent literature which indicates clearly that to understand the term "operationism" in any of its forms, specific attention must be paid to the way or ways in which the principle has been used by each writer on the subject. Otherwise, confusion results. The quest for the definition of the operational principle does not lead to unanimity of opinion.

Inasmuch as the proponents of operational meaning in psychology interpret the principle in divergent ways a more

detailed analysis of the actual application of operationism in selected experiments is pertinent. These more specific considerations may well be introduced by reference to Melton's (11, p. 317) critical analysis of the scientific method as related to the psychology of learning. This investigator holds that "the single measurement is a fallible indicator of the average conditions prevailing throughout a series of measurements, since it is partly, if not wholly, determined by variable factors." The bearing of the operational principle upon the question of uniqueness of any scientific datum is hereby specifically indicated. Melton goes on to point out that "*in so far as the measurement is not an exact indicator of the average conditions prevailing throughout the series of measurements it lacks reliability.*" The operational principle in relation to the problem of reliability is thereby suggested. "*In so far as it* [the single measurement] *is not an exact indicator of factors that the investigator identifies, but is, rather, an indicator of the effect of unidentified or erroneously identified factors, the measurement lacks validity*" (p. 317). The relationship between the operational principle and the validity of measurement becomes, it is believed, a pertinent problem. To these questions of interrelationship we now turn.

2. *The recurrence of phenomena and the operational principle.*—The relation of the operational principle to the problem of recurrence of phenomena will be discussed first. On the basis of one prevalent conception of lawfulness (Aristotelian) it is to be noted that inductive scientists deal exclusively with recurrent phenomena. They proceed to collect data which eventuate in making possible a series of probability analyses. Only in this way is it possible, from their standpoint, for prediction and control to be effected. In psychology the presumably large number of variables existent in the determining complex of any resultant event necessitates a series of progressively refined experiments each of which aims toward the isolation of the antecedent correlates. By virtue of the large number of variables which may be assumed to compose the stimulus complex, the successive analyses of these determining factors become of primary importance for prediction and control.

If we use the operational principle during the early stages of our isolation procedures—and we appear to be working in the early stages today—we may well curtail the number of progressively refined experiments each of which aims toward the isolation of additional factors. For example, the specific operational definition of tonic immobility, presented by one investigator (**8**) in terms of the gross complex of experimental factors which produce this behavior pattern, is pertinent. This study, it is believed, illustrates the fact that the existence of a number of non-specified stimulus variables tends to preclude the possibility of proposing any meaningful statement relative to the relation of tonic immobility to the *operation* of the retinal inversion in the Rhesus monkey studied. Yet this experimenter states "it is interesting to note that the present observations illustrate the experimental production of phenomenon through visual stimulation alone." The operations performed to induce this immobility have in this instance failed to make clear what factors (as, for example, emotional reactions measured by appropriate instrumentation) are correlated with the induction of a specific operationally defined response pattern.

Further, this definition, assumed to be operationally meaningful by its author, may tend to make impossible further analyses of the stimulating conditions. Thus, it is conceivable that this operationist's usage has merely slowed the formulation of additional studies which, when carried out, might lead to the isolation of the correlates of tonic immobility. Although this reaction might prove to be a recurrent phenomenon within the operational limits set by the present investigator, other experimenters in studying animals relatively more or less emotional in their behavior, might demonstrate the uniqueness of the data described above. This citation illustrates our point that the operational principle, as actually used by some of its proponents, may, by way of acting as a blanket interpretation, exaggerate rather than minimize the influence of probable error and so make for greater uniqueness. The principle may, when so used, partially close the door to further experimental refinement in which factor isola-

tions would otherwise be attempted. This uniqueness of the scientific datum, as we have intimated, obviously has bearing upon the problem of reliability and validity. Uniqueness may indeed prevent prediction from the data obtained from sources other than those upon which the earlier operationally phrased conception has been based (**11**). Even though the operational concept may in principle increase the uniqueness of a given datum, as Bridgman has shown, it must at the same time, to quote from Stevens (**15**, p. 227), "be public and repeatable." To fulfill these requirements, however, it might be held, operationism would lead to a plethora of description-definitions leaving psychology void of scientific generalizations.

An additional citation taken from the work of another investigator illustrates the same point from a slightly different angle. This investigator states:

> The set of properties that define 'pressing a lever' is thus *uniquely determined* (italics ours); specifying either fewer or more would destroy the *consistency* (italics ours) of the experimental result. It may be added that in the case of conditioned operant behavior the defining property of a class is *exactly that given by the conditions of the reinforcement* (italics ours) (**13**, p. 38).

This statement might be interpreted to mean that these operational definitions subsume the principle of reliability, since the property primarily involved in classifying 'bar pressing' behavior is consistency as defined by the 'smooth curves' which obtain as the result of reinforcement under a given set of conditions. Inasmuch as the 'set of properties' which define the 'operant' behavior 'is uniquely determined' it is difficult to see how they necessarily possess reliability even within the conditions of this particular experiment. The difficulties of meeting the methodological requirement of reliability within the present operational 'system' are demonstrated by the experiments of other investigators (**7**, **17**) who have found the absence of consistency in the behavior of the rat under similar if not identical experimental arrangements. Perhaps one clue to this discrepancy is the fact that the first

investigator appears to have selected those items which corresponded to some preformed concept or criterion. This interpretation is indicated by the author's statement (**13**, p. 38) that "there are flexions and pressings that are so unusual because of other properties that they do not fully *count as such*." As we shall later demonstrate, there is nothing in the technique of operationism which permits arbitrary selection of data in the establishment of definitive and generic standards for a given response.

That the above conclusion is not a misrepresentation is shown by the fact that certain proponents of operationism have indicated that each definition is unique. Bridgman in 1928, for example, states that each operation, such as in the measurement of length, should be uniquely specified. A multiplicity of terms could scarcely be avoided as has been indicated by Waters and Pennington (**16**). Later, in 1938, Bridgman (**4**, p. 121) points out that "no operation can be uniquely specified in all its details. We ultimately have to assume from sheer weariness, if nothing else, that it is not necessary to carry our specifications beyond a certain degree of refinement; the place where we draw the line is again dictated by experience." If each operationally phrased concept is more or less unique, and if we have a multiplicity of more or less disparate concepts, the present writers believe considerable difficulty might arise in the accumulation of sufficient data to assure the recurrence of events with a high degree of probability.

In summary, the single measurement and its operational conceptualization may camouflage unspecified factors which contribute to the events as defined. The mere existence of a multiplicity of unique concepts scarcely makes possible an analysis in terms of which all factors are identifiable. Inasmuch as certain proponents of the principle of operationism assume that which is unique is repeatable, the quest for recurrence may prove to be a will-of-the-wisp in psychology.

3. *The quest for reliability.*—According to Melton (**11**), the greater the variable error of the experimental data the greater the degree of uniqueness of the event under investigation.

Likewise it follows that the greater the variable error the less is the likelihood of obtaining consistent results with the duplication of the specific experimental arrangements. The application of the operational technique to responses made to experimental situations may result in the formation of operationally couched concepts which superficially appear to lend reliability to the data. This reliability, it is maintained, may well be more apparent than real; spurious reliability may obtain by reliance upon mere operational phraseology. By way of illustration, one investigator (**13**) has operationally defined differential behavior in rats in terms of an unanalyzed stimulus complex. No isolation of the exact stimulus variables precipitating the operationally defined response has been attempted. Superficial consideration might lead one to assume, in terms of this formulated definition of a behavior pattern, a high degree of reliability. The actual problem of the reliability of the instrument used, however, has not, as such, been raised. Other investigators (**7**, **17**), who have considered this methodological problem, have found the results obtained with a similar technique to be extremely variable. It is maintained in the present discussion that the operational principle contains no feature which supplies inherent reliability. Realization of the fact that unreliable data may be highly significant does not lead the authors to minimize this conclusion. Nor does the question of the meaning of reliability as considered in a general way by Carr (**5**) bear upon the inadequacy of the operationist's usage of this principle when he assumes the reliability of his methodological procedures within his interpretation of operationism. The quest for reliability must thus proceed beyond the acceptance of the operational principle as a technique by which private data are made public property. Definition in terms of operations fails to give reliability to that which is merely being put into words.

4. *The quest for validity.*—Psychologists agree that scientific determinations should be "true" measures of what they purport to measure. In order to determine the validity of various sets of data obtained by means of any measuring

instrument, psychologists commonly seek to determine the relationships of those data with certain arbitrarily selected validating scores. A case in point is the measurement of 'intelligence.' Little can be said in favor of any operationist's procedure of subsuming within the sphere of influence of the operational principle the concept of validity. When this is assumed, the validation of the instrument might be inferred to arise from the mere exercise of naming and defining. It is indeed doubtful whether the making of private data public can contribute validity. Although to define "intelligence" as "what the tests test" may render the formulation impregnable to attack, such a definition contributes nothing toward the validation of the scale *per se*. It may be concluded, therefore, that those who appear to subsume validity as an integral feature of operationism are assuming a non-existent validating criterion. No interpretation of the operational technique, it is held, can provide validity for the measuring instrument.

The arbitrariness with which certain validating criteria are posited in the formulation of operational definitions is illustrated below. Although Bridgman's fundamental formulation of the principle includes both the physical and the 'mental' operations, certain writers have arbitrarily accepted as valid selected physical data and have rejected the 'mental.' The latter, they assume, may not be defined operationally because corresponding physical operations cannot be designated. According to one writer (1) introspective analysis of 'temporal immediacy' proves to be a non-existent problem, since the existential data resist the formulation of an operational definition. Because 'immediacy' cannot be 'clocked,' it is said, there is no problem. "The problem (1, p. 522) seems to be what is introspectively obvious is not necessarily true." One may ponder why one set of operational definitions, phrased in terms of the clock, is preferred *a priori* to others by means of which 'immediacy' in time could be given meaning operationally. Felt 'immediacy' certainly might be defined in terms of the subjective operations reported by an observer as judgments. The criterion based on the clock has apparently been selected, however, as the 'true' one. Such

arbitrary selection indicates the presence of some preconceived criterion for the determination of what operations among others are valid. It is difficult, however, to understand, except of metaphysical grounds, how one can make one set of operations prepotent when the so-called subjective and objective procedures both spring from the same root, namely, the processes used in making public originally private data. Thus, it would appear that the utilization of the operational technique fails us in our quest for the validity of a scientific measurement. The operational technique yields no criterion for the determination of the "true" measurement.

5. *The quest for the concept.*—Scientists, again, are agreed that mere fact-finding is not enough. Most scientists declare their ultimate concern to be not only with discriminations and differentiations, as stressed by the proponents of operationism, but also, and even more particularly, with making generalizations on the basis of the data obtained. Certain it is that the advocates of the operational technique, if they wish to proceed beyond the level of description-definition, must formulate more clearly the means by which operationism makes generalizations possible. That the latter role of operationism has received scant attention is obvious from a survey of the literature. We are told, for example, by Stevens (**15**), that the operational technique makes possible the grouping of data into what may be called classes. But to accomplish this organization, certain operational criteria of a nature not clearly disclosed are essential. Nowhere have the authors found a demonstration of the establishment of such generic standards.

Thus, it would appear that the proponents of the operational method in psychology have failed to present in any precise manner a discussion relative to the formulation of concepts by recourse to operationism. Still, in spite of these apparent inadequacies, it is believed that this technique, properly conceived, may well prove useful. As Pratt (**12**, p. 121) has said, "science thrives on guesses . . ." though "too many guesses turn out to be duds." If we are to guess, that is, to attempt the formulation of higher order concepts,

then it is conceivable that the operational principle might well be employed. The principle can guide us, from the present point of view, in 'tying' our guesses to the gaps in our knowledge and can demand that these guesses be so formulated as to be capable of experimental test. By so doing the age-old attribute of the scientific method can continue to make for clarity and for scientific advancement even though disguised under a new nomenclature.

The proponents of operationism, in their emphasis upon making raw data meaningful, have seemingly failed to stress the technique as a guide in the formulation and delimitation of experimentally verifiable hypotheses. The principle as it is now used in psychology appears to facilitate the formation of exploratory studies rather than the conceptualization of critical experiments. It would seem, also, that certain adherents of operationism have been so concerned with the derivation of meaningful first order concepts that the purpose of this derivation has been forgotten. One cannot literally see the forest for the trees. Indeed, it is as important a function for operationism to aid in the formulation of hypotheses as it is for it to aid in making private data public. Why the proponents of operationism have stressed the latter role practically to the exclusion of the former may conceivably be that the uniqueness of each first order conceptualization makes description-definition less difficult. If each construct, on the other hand, is more unique than it is recurrent, as Bridgman (**2**) implies, then the absence of recurrence of phenomena tends to emphasize the role of description to the partial exclusion of generalization. Still, this interpretation cannot be said to be entirely adequate, since Bridgman (**3**) has pointed out more recently that no meaning can be operationally expressed in the absolute sense. Pratt (**12**, p. 111) has stated this clearly when he says "the construction of absolutely pure operational meanings . . . would be practically impossible, even if it were desirable. Purity of meaning . . . is always a goal, never an actuality." Why then emphasize merely one of the uses of the operational technique? Perhaps this stress is given because of the absence of any clear formulation of what

is meant by the principle of operationism. It may be said that the technique, as conceived by some of its adherents, is intentionally limited to the positivistic approach of description-definition at the level of the original data. The search for the operational "rules" by which the basic concepts are formulated ends, thus, in a failure to note any consistent means whereby the data may be grouped or classed.

In summary, the operational principle as currently defined and used by psychologists needs clarification. If the technique is to be utilized as a basic methodological tool, as its proponents demand, then psychologists should know not only the generally accepted meaning of the term, but they should be aware of its uses and limitations. In terms of the present interpretation we find no way by means of which operationism can contribute, as apparently assumed, reliability and validity to scientific verbalizations. How the psychologist can proceed by the operational technique alone from initial formulations based on the scientific data to generalizations basic in the framework of any science is not clear. It is believed that the proponents of operational meaning have overstressed the partial function of description-definition to the exclusion of the role which operationism might well have in the formation of hypotheses. Operationism, therefore, may be looked upon as a scientific tool or slogan by which the scientist holds himself in check during the formulation of his conclusions to any experimental or theoretical problem.

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A METHODOLOGICAL CONTRIBUTION TO THE NATURE–NURTURE DILEMMA IN TESTED INTELLIGENCE

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Now that the smoke of battle clears a little in the I.Q. sector of psychological measurement, it is possible for the detached observer to get some insight into the nature of the problem. To one who has no special stake in either the constant, or inconstant, features of tested 'intelligence,' it appears that whereas most people once assumed that tested intelligence measured the 'native' factor more than a 'nurture' factor, they now must concede that it is greatly determined by cultural influences to which the testee is exposed. There is nothing new or strange in this doctrine, and anyone who has thought through the implications of a measuring device whose most reliable part is the vocabulary test, will have realized years ago that aside from the broadest limits of idiocy and genius, the 'native' factor in intelligence may be effectively masked by differences in experience.

But it now looks as if this relative freeing of the tested I.Q. from the bounds of heredity is likely to prove a boomerang. For, if we cannot predict with any accuracy the inherent educability of a given person, the practical uses of I.Q. measurement are rather limited. Already one detects a certain worry in the ranks of the mental testers with talk of the 'disillusioned public' and their 'loss of confidence' in all psychological and educational investigations. Of course, one can save present I.Q. tests by adopting the attitude that he is only interested in measuring differences in cultural background so as to equalize opportunity. Or he may go even further, as would certain uncritical educators, and assert that the new evidence on growth of the I.Q. proves that heredity is not especially important in mental alertness and that, given

adequate time and tools, one can make a genius out of almost any unit of human protoplasm.

To the present writer, at least, it seems that the whole history of the human race denies such an assumption, and unless biologists take rigorous steps to combat its insidious implications, we shall be wasting even more public money than we now do in trying to bring all persons to an equivalent level of intellectual attainment. There is no 'fascism' lurking behind a more cautious doctrine, only the law of diminishing returns! It has been explicitly stated by even the most ardent supporters of the inconstancy of I.Q. scores,[1] that some human nervous systems presumptively can profit more by a given environmental opportunity than can others; the real dilemma in current controversy is that such relatively constant 'native' differences in ability are not well marked by present measuring devices. Let us frankly admit that (1) inherent differences exist in the 'educability' of human nervous systems, (2) present 'intelligence' tests do not measure these differences at all adequately, (3) the problem of measuring these differences should be attacked by new methods.

The writer wishes that more of the energy now being expended to make the old methods work, or to find out their detailed limitations, could be deflected into new channels of thought. The question of 'what next' is hard to answer. And yet, it seems that at least one promising avenue of approach has remained relatively untried. This is the development of tests of concept formation by use of 'nonsense' figures. Such tests should follow, in logical consequence, if it is fairly generally agreed among psychologists that the distinguishing feature of intellectual performance is a highly refined discriminative sense for abstracting common elements or relations in 'new' situations. Discrimination at the conceptual level is already involved in several existing intelligence tests, but the failure to standardize the experience upon which such concepts rest and the use of language in expressing such concepts prevents these tests from revealing predominantly any 'native' acuity.

[1] B. Wellman, Our changing concept of intelligence, *J. Consult. Psychol.*, 1938, 2, 97–107; G. Stoddard, The I.Q.; its ups and downs, *Educ. Rec.*, 1939, 20, 44–58.

Nonsense figures such as introduced by Moore,[2] Fisher,[3] Hull,[4] and especially by Smoke,[5] seem to lack this defect. Combinations of straight and curved lines can be drawn so as to present forms never before experienced and these different forms can be arranged in concept 'families' and given such nonsense labels as 'deral,' 'wez,' etc., according to the presence of common symbol elements or common interrelations of elements. A short standardized training series having both negative and positive instances of each family would be followed by a test series, utilizing 'new' specific instances of previously illustrated concepts. It should be possible to assemble a graded[6] series of these nonsense-figure concepts which would be as 'foreign' to a person of wide cultural attainments as to an illiterate. Degree of individual progress from simple to complex concepts might thus have rather constant diagnostic significance. Work with various criterion groups, including amentias and dementias, would further strengthen this position.[7] And, if it were found that ability in discriminating nonsense concepts differed with chronological age, norms could be established after the manner of the original Binet technique. Such a program is, of course, a life's job for several individuals, and it should not be attempted unless preliminary work reveals that the nonsense-figure test is much less influenced by differences in cultural

[2] T. V. Moore, The process of abstraction: an experimental study. *Univ. Calif. Publ. Psychol.*, 1910, 1, No. 2, pp. 124.

[3] S. C. Fisher, The process of generalizing abstraction, and its product, the general concept. *Psychol. Monogr.*, 1916, 21, 1–213.

[4] C. L. Hull, Quantitative aspects of the evolution of concepts. *Psychol. Monogr.*, 1920, 28, No. 123.

[5] K. L. Smoke, An objective study of concept formation, *Psychol. Monogr.*, 1932, 42, No. 4, pp. 46.

[6] Such grading could be achieved by defining difficulty operationally in terms of the number and kinds of inter- and intra-group relationships between elements used in isolating a 'family' concept, or difficulty might be defined in terms of the percentage of persons of a given age successfully solving a given concept.

[7] Using an essentially similar method, Yerkes and his co-workers showed that ability to discriminate new specific instances of previously developed abstractions follows roughly the phylogenetic order of neural growth. For a summary of these studies, *cf.* N. R. F. Maier and T. C. Schneirla, *Principles of Animal Psychology*, New York: McGraw-Hill, pp. 458–460.

background, such as amount and kind of school experience, than present intelligence ratings. It is suggested that study of the relative influence of nursery school attendance on development of concept scores and I.Q. scores might be taken as symptomatic of the new test's value.

The past decade has been noteworthy in showing the limitations as well as the values of present intelligence tests. But let us not blind ourselves to the absence of a native factor simply because our existing tests are so heavily weighted with the experience factor. Let those who are disillusioned with existing tests use their chagrin to motivate the search for new and better methods of measurement. And until the results of such attacks are in hand, let us all remind ourselves that the years of research in mental measurement which lie behind have not proved anything definite about the relative importance of nature to nurture in discriminative intelligence, but only that existing methods are not adequate to the problem.

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